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Philosophy of Science

VOL. 4 April, 1937 NO. 2

The Meaning of Simplicity in Physics

BY

R. B. LINDSAY

IN THE fourteenth century William of Occam in the course of his attack on the medieval scholastic philosophy enunciated his famous "razor": *Entia non sunt multiplicanda praeter necessitatem* (hypotheses should not be multiplied unnecessarily). This is the classic claim for the description of nature in terms of the minimum possible number of fundamental concepts. It was presumably so recognized by Newton in the third book of his "Principia" in 1687 when he wrote: "We are to admit no more causes of natural things than such as are both true and sufficient to explain their appearances. To this purpose the philosophers say that Nature does nothing in vain, and more is in vain when less will serve; for Nature is pleased with simplicity[1] and affects not the pomp of superfluous causes."

There is little doubt that the progress of science during the 18th century was colored by the feeling that nature *is* simple. Perhaps our forerunners of the last century were not so sure. How do we feel about this matter today? Is nature really simple? What does this phrase mean to the modern physicist? The present article is concerned with these and related questions.

[1] Newton's Latin reads "Enim Natura simplex est."

A study of the history of physics clearly indicates that the first impression of any natural phenomenon is almost always one of disorder and confusion. Only gradually do the careful and thoughtful investigators transform chaos into order. Certain aspects ultimately emerge as being of particular significance and are seized upon to the exclusion of others which are felt to be irrelevant. The Greeks must have contemplated the behavior of immersed bodies for ages before Archimedes hit upon the really pertinent facts and announced the laws of hydrostatics. A similar situation is seen in the discovery of the principles of motion by Galileo. Now the ability to describe large sequences of phenomena in terms of a single principle makes a great impression on the human mind and it is very difficult to resist the feeling that there is a reason for this success: namely that nature is essentially simple. So there grows the conviction that the true laws are simple laws in the sense that they are expressible in terms of a few symbolic concepts and a few elementary mathematical operations. From this point of view the truth of laws like those of Boyle and Hooke is seen to be guaranteed by their particularly simple functional form; this actually was a common argument during the early nineteenth century. It does not strike us so convincingly today, for we recall that whenever we probe more deeply into the phenomenon described by the "simple" law the elementary formula no longer describes adequately the new details and must be replaced by something more elaborate unless the entire fundamental viewpoint is changed.

For example, the familiar falling body formula $s = \frac{1}{2} gt^2$ may appear at first sight to insure the essential simplicity of motion near the surface of the earth. More careful investigation, however, shows that the formula is not strictly true, for it does not account for the observed small eastward deflection and other peculiarities of the fall. To describe the latter, more complicated equations are necessary. This does not dim the ardor for simplicity, however, for ultimately some clever person observes that the difficulty is due to our use of a system of reference which, being fixed in the earth, is rotating. The "simple" formula holds in a non-rotating frame, which to be sure is rather unnatural for

us on the earth to employ. We exchange the simplicity of a formula for the simplicity of a more fundamental concept: it is impossible for us to have our cake and eat it too.

There are many illustrations of the situation where a so-called "simple" formula acts as a cloak for a description of nature which many physicists consider to be actually very complex. This, for example, is the case with the celebrated quantum theory relation $E_1 - E_2 = h\nu$. No one doubts the huge success of this analytically very simple formula. There are still many, however, who feel that it conceals a mystery which will eventually be clarified and explained in "simple" terms, e.g. by an appropriate modification in the basic point of view.

The major difficulty encountered in discussing all these things lies, it is clear, in the lack of precision in the meaning of the concept of simplicity in physics. We wish therefore to examine various possible definitions, their advantages and shortcomings.

One trivial way to end the discussion at the start would be to agree to see in simplicity nothing but the basic assumption of the uniformity of nature on which the possibility of all science rests. Then all physics is *per se* simple. It is difficult to be contented with this disposition of the problem. Let us try something less tautological.

In 1874 Kirchhoff, in the introduction to his lectures on mathematical physics, defined mechanics as the subject whose task it is to describe completely and in the most simple manner the motions occurring in nature. The fundamental emphasis on the idea of simplicity in this definition is suggestive. The implication is strong that the success, nay even the truth of mechanics, depends largely on its simplicity. This comes very close to associating simplicity with truth: if we only knew what is true, we should automatically know what is simple and vice versa. But how are we to know what is true? It is more than probable that Kirchhoff took for granted that every physicist knows what simplicity is; unfortunately this is just the problem we are investigating. Other writers on the foundations of physics, e.g., Mach and Poincaré, seem equally to take the general understanding of the meaning of simplicity in physics for granted.

Recently indeed, Reichenbach[2] has criticized Kirchhoff's apparent tendency to emphasize the close connection between simplicity and truth by introducing two separate notions to which the name simplicity may be attached. The first, *descriptive* simplicity, is illustrated by imagining the existence of a number of equivalent theories which may be used indifferently in the description of a given set of phenomena. There is no meaning to the question: which theory is true? For certain purposes, one may be more convenient than another. Thus for most practical optical calculations the ray theory of light is more convenient than the wave theory, although all the facts can be adequately described by either theory. As far as these phenomena are concerned geometrical optics may be preferred because of its simplicity, which here is presumably to be interpreted as *convenience.*

Before criticizing this notion let us pass on to Reichenbach's second kind of simplicity to which he ascribes the name *inductive.* This is exemplified by the ordinary practice of the physicist of drawing a smooth curve through the discrete set of points used for the graphical representation of experimental observations. Reichenbach considers the feeling for simplicity inherent in this tendency as more fundamental than the descriptive one. It is hardly misinterpreting him to suspect that he considers inductive simplicity as closely associated with truth value. The continuous curve is assigned a greater truth value because of its greater probability, which is thus used as a criterion for truth and at the same time for simplicity.

When we probe a little below the surface it is difficult to see any fundamental difference between the two definitions. Certainly there is no definite assurance that the continuous curve we draw through the discrete points describes the observed phenomena more truthfully than do the points themselves. It is granted that the mathematical technique involved in working with the continuous curve is more familiar to most than that involved in working with the discrete points. But this does not make the curve more probable or more truthful unless we are willing to base probability and truthfulness on convenience. This

[2] Handbuch der Physik, Vol. IV (Berlin, Springer, 1929) p. 34.

course, however, would reduce "inductive" simplicity to the same level as "descriptive" simplicity. Indeed it is possible to argue that the drawing of the smooth curve is *a priori* an unwarranted extrapolation. We do it simply because it works, i.e., the resulting description agrees often enough with experience to satisfy us.

What do we mean by saying that one procedure or one theory is more convenient than another? Perhaps it is an instinctive and not analysable concept. Curiously enough what seems convenient to scientists of one age appears the reverse to those of another: to treat light as a wave phenomenon in an elastic solid appeared convenient to the physicists of the nineteenth century; it has long since ceased to appeal to our contemporary age. In so far as simplicity is measured by convenience it alters with time. The idea of convenience is without doubt closely associated with Mach's "economy of thought" concept, though this idea itself demands more study than has been given to it.

The foregoing brief critique indicates the desirability of exploring further the possible meanings that may be attributed to simplicity in physics.

Physics is a description of a portion of human experience. The simplicity of physics is thus essentially simplicity of description. What do we mean by a simple description? Certainly this question would be answered differently by a naturalist and a physicist. The naturalist depicts the structure, behavior and relations of animals by means of a system of classification into types; this is of such a nature that a careful examination of any particular animal enables him to fit it into its proper place in the system. The method of description employs a specially invented technical vocabulary, but aside from this the language used is that of everyday speech. Presumably the naturalist considers his description simple if it replaces the complexity of animal life apparent to the untutored observer by a classification embodying the smallest possible number of essentially different types. And his search for simplicity presumably becomes the search for the method of classification which meets this requirement.

Now physics is not a classificatory science and in its ultimate form, namely theoretical physics, uses largely the symbolic lan-

guage of mathematics in place of the language of common speech. A symbol is assigned to represent each physical concept, and the laws of physics are the relations among these symbols which are verifiable by experience. Physics is deemed to have solved its task satisfactorily if it is possible by the introduction of suitable postulates (the principles of physical theories) to deduce purely mathematically the laws which experience confirms. But here is the nub of the matter: when shall we say that this process is simple?

It is presumably sensible to examine each element of the process from the standpoint of simplicity. These elements are: (1) the fundamental concepts, (2) the postulates, (3) the mathematical methods for deducing laws from the postulates.

Of all physical concepts, what are the simplest? The physicists of the nineteenth century would have betrayed no hesitation in answering: those most closely associated with primitive sense perceptions, viz., space, time and number with the attendant properties of the three dimensionality of space, its Euclidean geometry, etc. Not all our predecessors of the past century would have insisted that these concepts are intuitive and *a priori*, but most of them held that they flow naturally from the primitive sense data. It was therefore natural for them to believe that a physical theory constructed solely from such concepts is simple. Of all physical theories, classical mechanics most closely fulfills the assumed criterion, since it operates with concepts most immediately connected with ordinary experience. The conclusion to be drawn seemingly is that in order for physics to be simple it should rest on a mechanical basis. Hence the high favor bestowed on mechanical theories during the eighteenth and most of the nineteenth centuries. Even today there are not lacking voices that lament the "complexity and abstractness" of quantum theory and plaintively long for the fundamental "simplicity" of the mechanics of Galileo, Huyghens and Newton. After all, classical mechanics is "understandable!"

Unfortunately the story of the failure of classical mechanics in the description of atomic phenomena is but too familiar. If only the atom had not pushed its way into the fore front of physi-

cal speculation all would have been well. But this is only another way of saying: if only physicists were to confine their investigations to the macroscopic and stop short of the microscopic domain, the classical theories would still satisfy! There is indeed small reason to doubt the possibility of a purely mechanical description of all physical phenomena, but the price of this seems almost prohibitive. At any rate the attempts so far made in the atomic domain have led to what most physicists agree is unnecessary and undesirable complexity. It is therefore evident that there exists no reason for maintaining the view that simplicity resides essentially in mechanical concepts. Perhaps it would be fairer to say that while such concepts may appear most simple in the sense of being closer to direct experience, the whole process of physical reasoning using them becomes too complicated as soon as the macroscopic domain is transcended.

To clarify the discussion of this point it will help to examine a particular concept which is of importance in all physical theories, viz., the "state" of a physical system, and contrast its meaning in classical mechanics to that in quantum mechanics. Classically, in the case of a system of particles the state is said to be specified by the coordinates and momenta of the particles in some reference system. Usage here is not entirely standardized, for a knowledge of the coordinates alone is often said to fix the *configuration* of the system while knowledge of both coordinates and momenta specifies the *phase*. The important thing to note is that the phase or state, classically conceived, is essentially a set of numbers attached to symbols representing directly mechanical concepts which flow more or less immediately from primitive space and time sense data. These numbers and the state they specify may alter with the time. Even in the case where the dynamical method is applied to problems in which motion is not directly observed, e.g., the dynamical theory of electric currents, the state of the system, i.e., the circuit under consideration, is given by a set of directly measurable quantities viz., the charges and currents. The classical concept of state is therefore very closely related to crude experience.

Much more abstract is the meaning of state in quantum me-

chanics. Here the state is specified by a mathematical function of certain variables, viz., space and time coordinates or transforms of these. The function itself is not a mechanical quantity, at least in the familiar sense. Rather it is a kind of "source" function from which mechanical quantities can be calculated by means of a fundamental formula of quantum mechanics. We do not need to present a detailed analysis. We shall only emphasize that the two concepts of state are on different levels of abstraction. Who is to say, however, which is the more simple? On the ground of familiarity alone many physicists would naturally prefer the classical concept. Since, however, anyone who wishes to take the trouble can understand the quantum mechanical concept sufficiently to use it with success, any preference here based on an assumed *a priori* simplicity of classical mechanics appears decidedly illusory.

The tentative conclusion reached at this point is thus that it is impossible to find a criterion for simplicity in the fundamental concepts of physical theories. But it may be objected that we have not exhausted the possibilities here. Every physical theory rests on a plurality of concepts. Can we not say that the simplicity of a theory is measured inversely by the *number* of *independent* concepts necessary for its establishment?

A striking illustration of the plausibility of this judgment would appear to be provided by the theory of light in a comparison of the corpuscular theory of Newton with the wave theory of Huyghens, Fresnel and Maxwell. The former operates with corpuscles or small particles emitted in every direction from luminous bodies. These corpuscles move through an ethereal medium pervading the universe, including also all matter. When corpuscles impinge on the surface of a body the difference in the ethereal medium inside and outside is invoked to account for the deflection of the light ray, which takes place toward the normal if the velocity of light is greater inside than outside. At the same time the incidence of the corpuscles excites vibrations in the ether and this provides a mechanism for the absorption of light by the body in question. Toward the end of his life Newton inclined more and more to discard the ether and get along with the particles

alone. Now the light corpuscle is undoubtedly suggested by the concept of the material particle of classical mechanics and it would have been very satisfactory if the phenomena of optics could be described merely by endowing the corpuscle with extrapolations of the ordinary properties of such a particle, viz., vanishingly small size and inertia, very large velocity and the power of attracting or being attracted by material particles. Unfortunately Newton found it necessary to attribute to the corpuscle additional properties apparently very different from those of observed material particles. For example, he had to suppose that the corpuscles when incident from a vacuum on the surface of a body are attracted by a force which acts only along the normal to the surface. Moreover, in order to account for the color phenomena associated with the incidence of white light on thin plates he had to assume the possibility of the corpuscle getting into a kind of transient state disposing it to be easily transmitted or reflected, (recall the so-called "fits of easy transmission and reflection"). Finally in order to account for polarization he was forced to postulate that "every ray of light has two opposite sides," etc. All these assumptions involve the introduction of what can only be considered *independent* concepts foreign to the nature of the material particle of classical mechanics.

The situation appears to be much improved in the wave theory of light. The ordinary wave concept of the classical mechanics of media suffices, without the addition of gratuitous postulates, to describe not only reflection and refraction but also the diffraction, interference and polarization of light. To be sure difficulties arise when the properties of the medium are investigated closely: the elastic solid constituting the luminiferous medium has to be more elastic than steel and yet so tenuous as to oppose no observable resistance to the motion of the planets. It might seem that this theory like the corpuscular theory sins by the invocation of extra concepts not contemplated in the classical theory of media. However, this particular trouble was obviated by the reminder of Stokes that even certain ordinary substances, e.g. pitch, are comparatively rigid to sufficiently rapid deformations, but oppose little resistance to slow deformation. The concept

of the elastic solid light-bearing medium would thus appear to be a reasonable extrapolation of the elastic solid of ordinary experience. A more serious problem is provided by the necessity of getting rid of the longitudinal wave which exists in disturbed elastic solids but which is not observed in optical phenomena. This is not the place to enter into the long history of the attempts to circumvent this embarrassment. It is sufficient to remark that the problem was finally solved only at the cost of considerable elaboration which most physicists would today regard as the sacrifice of simplicity. As a matter of fact by the time the problem was solved the elastic solid theory of light had been shelved and its place taken by the electromagnetic theory. This brought to the fore a quite different, independent concept, the electromagnetic field. As applied to light the fundamental idea of both theories is that of wave propagation, and judged by the criterion of simplicity mentioned above it is hardly to be doubted that they must both be regarded as simpler than the Newtonian corpuscular theory. If the questions raised by the medium (either elastic or electromagnetic) are brought forward to dispute the credibility of this statement the reply may be made that in the way in which the wave theory of light is actually used in the discussion of the elementary phenomena of reflection, refraction, diffraction, interference and polarization (dispersion and absorption are here expressly omitted) the properties of the medium as such do not enter in any vital way, the behavior of waves in general being sufficient for the description. In other words one does not have to postulate for light waves special properties in addition to and foreign to those of waves in material media.

The purpose of this somewhat lengthy summary of the theories of light has been to emphasize what may be called for short the "minimal-concept" or numerical criterion of physical simplicity. Another illustration has been mentioned and commented on by H. Margenau.[3] This is derived from a comparison of the 18th century caloric theory of heat with the more recent mechanical theory. It is at once observed that the caloric theory sins against the above mentioned criterion by being forced to ascribe to the

[3] H. Margenau, Phil. Sci., 2, 48, 1935.

heat substance, caloric, the property of creatability, not contemplated in the ordinary physical properties of matter. This is an additional independent concept. It is rendered superfluous by the assumption that heat is a form of energy to which the conservation law applies.

Other illustrations of the "minimal-concept" criterion can doubtless be produced. However, in the examination of them a certain fundamental question arises. Just what meaning is to be attributed to the term "independent concept?" In the above example from the theory of light there seems to be no doubt that the average physicist will be satisfied to consider the possibility that a light corpuscle shall get into a "transient state," a concept foreign to those which classical mechanics utilizes. To this extent it is fair to treat it as an "independent" and unrelated concept. The same can be said of the hypothesis of the creatability of caloric. Let us, however, look at the matter more closely. For the sake of explicitness we confine our attention to mechanics.

Now material particle, displacement, velocity, acceleration, force, mass and energy are mechanical concepts.[4] Are all these independent? Let us examine this question by reviewing the meaning assigned to these various concepts.

In mechanics the material particle is a concept defined in terms of certain properties. Thus effectively a material particle has all the properties of a geometrical point in Euclidean 3-dimensional space in addition to two others of a non-geometrical nature, viz., (1) that of having certain relations to other particles, so that its motion is in general dependent on its position relative to other particles, and (2) that of inertia, the measure of which is its *mass*. These concepts become precise only when described in terms of the kinematical concepts of displacement, velocity and acceleration.[5]

[4] In the interesting paper of Margenau, above cited, on the Methodology of Physics, the term "construct" is introduced in place of "concept," presumably to emphasize the active constructive rôle of the mind in building physical theories. This is a worthwhile procedure. However, the present paper will continue to use the more familiar term with the feeling that no essential ambiguity will result.

[5] See Lindsay and Margenau, "Foundations of Physics," (New York, 1936) p. 79 ff.

The question now arises as to the mutual relation of displacement and velocity. If we consider that *time* and *space* are both necessary fundamental elements in which mechanical description is to be carried out, then all displacement must take place in time and the concept of velocity is not independent of or unrelated to that of displacement. It is idle to argue that the precise symbolic form in which velocity is expressed (i.e., whether distance divided by time or time divided by distance) represents a choice not dependent on the concept of displacement. The fact remains that once motion is admitted to be something which takes place in both space and time the concept of displacement is logically unthinkable without a corresponding concept relating displacement with time and the precise manner of defining this latter concept is a matter of indifference. It seems that the same argument must hold for acceleration and indeed for all higher time derivatives of displacement which might be introduced into mechanics. Whether time and space are themselves unrelated concepts is a philosophical question into which we do not intend to go.

When we come to the assumed non-geometrical properties of the material particle, it is seen that the assumption that the particle has certain relations to other particles is an essentially *new* property from those of position and change of position alone. Mechanics conceivably might operate with swarms of particles having no relation to each other (where, for example, only the number per unit volume might be of significance). Actually it does not do this. Moreover the precise assumptions underlying the definition of mass are also independent of the purely kinematic concepts.[6] On the other hand on the basis of the foundations of classical mechanics as discussed in the book just mentioned the concept of force cannot be considered as independent of those of kinematics, the assumed interrelations of particles and mass. It is perfectly conceivable to develop the whole of mechanics without explicit reference to force (cf. Gauss' principle of least constraint and Hertz's development of the theory of mechanics). The same situation is met with in the concept of *energy*. When

[6] Cf. "Foundations of Physics," p. 91.

we introduce these concepts it is not so much via new assumptions as merely baptizing certain combinations of symbols which occur frequently and therefore seem to merit special attention and use. This is quite a different thing from independence in the usual sense. Naturally the viewpoint taken here excludes the introduction of force and energy as anthropomorphic concepts wholly outside the purely logical development of mechanics.

It seems clear then that in any classical mechanical theory of a realm of physical phenomena there is a definite number of independent concepts. Any additional assumptions about material particles transcending the fundamental principles of mechanics, such as, for example, the assumption of the creatability of particles out of nothing or the destruction of particles, are then genuinely foreign, independent and unrelated concepts. As another illustration consider the quantum conditions of Bohr's atomic theory. These introduce into mechanics the concept of the quantum of action which is not at all contemplated in the fundamental classical principles. On the other hand how stands it with the usual initial and boundary conditions of mechanical problems? These appear merely as specifications appropriate to render definite the solution of the equations of motion for a particular problem. They cannot be considered as entailing anything independent of the fundamental concepts. The same may be said about the choice of the Hamiltonian function for a dynamical system. The principles of mechanics lay down no rules for the choice of such functions beyond the usual purely analytical requirements of differentiability, etc. The choice of a particular Hamiltonian can therefore not be considered an unrelated assumption grafted on to the fundamentals.

Now it is usually stated that all physical phenomena can ultimately be reduced to a description in terms of classical mechanics. If this is really the case the number of fundamentally independent, unrelated concepts will be the same for all such theories and on the numerical criterion of simplicity they are thus all equally simple! It is very doubtful that many physicists would be willing to accept this as a sensible conclusion. Hence what we have called the minimal-concept criterion of simplicity

becomes illusory unless one wishes to deny the validity of the statement made at the beginning of the paragraph. Thus, for example, one may actually take the attitude that it is really impossible to provide a genuine classical mechanical description of atomic phenomena. One then looks for a completely different construction and finds it, let us say, in quantum mechanics. This is of course a very proper procedure but very question-begging so far as a decision on the numerical criterion of simplicity is concerned. If we still believe in the criterion, we are faced with the fact that quantum mechanics operates with the concepts of classical mechanics but adds to them the wholly foreign concept of the ψ function and the reinterpretation of the concept of "state" as we have already previously observed. On this basis one is forced to confess that it is *necessary* to forego the simplicity of classical mechanics for the sake of describing new and more complicated phenomena. This will probably not impress an enthusiastic quantum physicist. He would probably prefer to conclude that the numerical criterion of simplicity is meaningless and that simplicity is something which can not be measured but which must be felt intuitionally—in one's bones, as it were.

As we think further about the matter we are compelled to believe that the numerical criterion of simplicity encounters still another difficulty. It is this: This criterion takes no account of the relatively great difference in nature of different concepts. It is perfectly conceivable that two theories may operate with precisely the same number of concepts, yet those of the one may be relatively much more abstract than those of the other. For example, the quantum mechanical concept of state is more abstract than that of classical mechanics, in the sense that the former has less relation to actual human experience in the large. How really can we compare from the standpoint of simplicity such radically different ideas? It seems futile to endeavor to do so.

We therefore reach the conclusion that there is little or no satisfaction to be obtained in attempting to use the numerical simplicity criterion in the concepts of physics. We have already mentioned that the process of physical reasoning includes not only the concepts but also the postulates which relate the con-

cepts and the mathematical methods of manipulating the postulates in the deduction of physical laws. Now the postulates in a very real sense serve to define the meaning of the concepts and consequently most that has already been said about simplicity in connection with concepts applies equally well to simplicity in connection with the postulates. How, for example, is one to compare decisively the assumptions of the kinetic theory of gases with the principles of thermodynamics? The former are so specific and the latter are so general that they appear of an entirely different character altogether. Any merely numerical comparison appears to be utterly out of the question.

The same inference appears to be even more plainly indicated in an examination of the mathematical analysis used in expressing physical theories. To be sure, it might be considered more properly in the province of the mathematician to speak on simplicity in mathematics. Nevertheless it must strike most physicists that in the progress of mathematics from algebra and geometry to higher analysis new concepts have been steadily added and whatever gain there has been in simplicity has been largely in the direction of economy and utility for the sake of which the scientist has been willing to pay the price of increasing abstractness.

Since the preceding analysis has been rather destructive of the purely numerical criterion of simplicity in physics, it is only fair to try to meet the question of the meaning of this idea on more positive grounds. No one denies that physicists continually refer to one theory as simpler than another. They must mean something by this. If this something cannot be reduced to a strictly numerical basis, what then does it mean? It will hardly do to dismiss it as utterly illusory.

We are here strongly tempted to return to the "economy of thought" stressed by Mach as one of the great contributions of scientific method to human activity. With its system of principles and laws physical science makes possible the intellectual mastery of nature to an extent inconceivable to the mind accustomed to look upon each natural event as unrelated to every other one; it makes it possible to teach an intelligent, industrious college student within a year to solve with ease problems which would

have given considerable difficulty to Aristotle and would very likely have been solved incorrectly by him, if solved at all. The tremendous practical applications of science all attest the marvelous economy of mental effort inherent in the method of physics. It is just as if the human mind in the process of evolution were always striving to achieve an understanding of natural phenomena in the shortest possible time and with the least mental effort, i.e., in a way which does not involve the retaining in the memory of an enormous number of isolated facts but replaces all these by a few great generalizations to be manipulated and elaborated at will by means of logical reasoning.

Applying this notion to our present problem it thus appears that we can find a criterion for simplicity in physics which may not turn out to be illusory by recourse to the following pragmatic judgment. Of two theories descriptive of the same range of physical experience that is the simpler which demands the shorter time for the normally intelligent person to become sufficiently familiar with it to such an extent as to obtain correct and useful results. No question is here involved of the precise type of concepts employed, of their number, of the number of fundamental postulates or finally of the special mathematical schemes employed in making deductions. If, for example, a person familiar with classical mechanics can become equally well acquainted with another physical theory in a time of the same order of magnitude as that which he took to learn mechanics, he should consider this new theory as simple as mechanics, no matter how complicated it may seem at first examination to one unfamiliar with it. Human life is short and time is fleeting. In endeavoring to understand the world around us we must make the most of the brief span allotted. We must therefore build our theories in such a way that, with given intellectual background, the manipulation of these theories leads in minimum time to success in physical prediction.

It must, of course, be confessed that this disposition of the meaning of simplicity in physics leads to the result that due to individual differences among scientifically minded people simplicity must ever remain essentially an individual judgment: a

theory which is simple to one will not appear so to another. Nevertheless past experience would tend to show that gradually as a matter of successive approximations points of view have arisen which are more readily assimilable by normal human brains, i.e., more readily learned, than earlier ones. These then have become part of the more or less "stable" and at least "quasi-permanent" body of physical thought. There is no reason to doubt that this process will continue. As new theories arise for the description of newly acquired phenomena they too by the great sifting process forever going on will take their proper place. And in turn we shall ultimately consider them simple when we have grown sufficiently familiar with them to forget that they ever seemed difficult to understand.

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Some Dangers Connected with Mathematical Applications

BY

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OF RECENT years there have appeared in the literature several attempts to apply to the subject matter of chemistry mathematical theories which had not thitherto found any use in that field. With the spirit and purpose of such attempts, the author of this paper is thoroughly in sympathy. But some of the investigators who have undertaken this kind of pioneer work in applied mathematics have apparently entered upon their tasks without appreciating the difficulties involved, and have hence produced results more likely to obscure than to clarify their subjects.

The two most important of these difficulties are the following:

(1) In introducing to a non-mathematical audience the terminology of a mathematical theory, it is necessary to be meticulously and almost pedantically accurate in the choice of words. If this precaution is not observed, some technical mathematical term is apt to be understood in its ordinary sense; and since this sense is frequently quite different from the mathematical one, the greatest confusion of thought may be produced.

(2) Before applying the precision methods of mathematics to any field of natural science, it is necessary to be sure that the concepts of that field can be defined with sufficient accuracy to permit such application. Many scientific terms have but a vague significance. They are practically useful in spite of the

fact that scarcely any two scientists would define them equivalently. But the advisability of applying mathematical reasoning to an idea stated in such terms is more than questionable. The results are usually expressed in mathematical terminology which to most readers implies an exactness and certainty unwarranted under the circumstances. Thus vague or even erroneous conclusions are clothed with a deceptive precision and a false dignity. The unfortunate nature of the consequences is too obvious to require discussion.

A recent paper by E. Habermann[1] entitled "Mengentheoretische Betrachtungsweise in der Chemie" well illustrates what is apt to occur when the above-mentioned difficulties are ignored. His work is an attempt to apply the methods of the theory of sets (Mengentheorie) to certain problems of chemical significance. One of his conclusions which will here be discussed in detail is: The whole set of hydrocarbons has the ordinal number ω^2.

This conclusion, taken as it stands, is astounding. If the ordinal number of the hydrocarbons is ω^2, their cardinal number must be aleph zero. And how can anyone expect to obtain by a priori mathematical reasoning an answer to the purely empirical question: What is the cardinal number of the hydrocarbons? But examination of Habermann's work shows that, as a matter of fact, he is not at all concerned with the hydrocarbons. His paper treats of formulae constructed according to the rules adopted for the construction of formulae which actually apply to known hydrocarbons. It is well known that very probably there is no one-to-one correspondence between such formulae and real hydrocarbons. In the first place, repeated attempts to prepare compounds corresponding to some of these formulae have failed. In the second place, the number of such formulae is easily shown to be infinite; and if the number of hydrocarbons is infinite, the number of atoms in the universe must be infinite —a conclusion vigorously opposed by some able cosmogonists.

In order to put Habermann's conclusion in more accurate form, it should be restated, for instance as follows: The whole set of hydrocarbon formulae constructable according to the ordinary

[1] Actualités Scientifiques et Industrielles, No. 392, Hermann, Paris, 1936.

chemical rules has the ordinal number ω^2. In this form, the statement might perhaps be acceptable to adepts in the theory of sets, but it is apt to produce a thoroughly false impression on the minds of most chemists. The cardinal number of hydrocarbon formulae is aleph zero, and any set with this cardinal number can be arranged in linear order in an infinite variety of ways. According to some of these ways, the ordinal number of the set will be ω, according to others 2ω, according to others ω^ω, etc. An infinite variety of ways gives to the set the ordinal number ω^2. Hence, in order to avoid misunderstanding, Habermann's conclusion should again be restated in some form such as the following: The whole set of hydrocarbon formulae constructable according to the ordinary chemical rules can (in an infinite variety of ways) be arranged in linear order so as to give to the set the ordinal number ω^2. As thus stated, the conclusion seems acceptable, but it is also trivial. For it is the direct consequence of the fact that the cardinal number of the formulae in question is aleph zero—a fact so obvious to any chemist that the proof may be omitted.

Habermann, however arrives at other conclusions with regard to the ordered set of hydrocarbon formulae which cannot be discussed without a slight digression. Let s_i and s_j be any two distinct members of the set S (with the cardinal number aleph zero) which is arranged in linear order. According to the ordering of S and the choice of s_i and s_j, the number n of members of S lying between s_i and s_j will be either finite or infinite. The ordering may be such that

(1) n is infinite for every pair $(s_i\ s_j)$.

(2) n is finite for every pair $(s_i\ s_j)$.

(3) n is infinite for some pairs but finite for others.

Only orders of the third sort will be here considered.

Suppose S to be so ordered, and suppose a proper subset S_a (with cardinal number > 1) to be chosen from among the members of S in such fashion that n is finite if both s_i and s_j are members of S_a, but n is infinite if only one member of the pair $(s_i\ s_j)$ is a member of S_a. Whenever a set with the cardinal number aleph zero is ordered according to the ordinal number ω^2, it is

possible to pick proper subsets S_a, S_b etc. of S such that these subsets meet the above requirements.

It is clear from Habermann's discussion (though he does not put the statement in succinct form) that, when the set S of hydrocarbon formulae is arranged according to the ordinal number ω^2, he considers that it is possible to divide S exhaustively and exclusively into proper subsets S_a, S_b etc. of the kind indicated above, and that, when S is so divided, each subset S_i corresponds to what the chemists call a homologous series of formulae. But, as has been stated, any set with the cardinal number aleph zero can be arranged in an infinite variety of ways so as to give it the ordinal number ω^2. It is evident that, of this infinite class of arrangements, Habermann is thinking only of a proper subclass, but just what subclass he has in mind is not definitely indicated. His position might therefore be put as follows: If the complete set of hydrocarbon formulae is arranged in linear order so as to give it the ordinal number ω^2, in at least one of the infinite number of arrangements which meet this condition, each member of S belongs to one and only one subset S_i (as defined above), and every such subset corresponds to a homologous series of hydrocarbon formulae.

Even in this form, however, the statement is open to grave objection. So far, chemical formulae have been spoken of as though there were only one set of rules for their construction. But it is well known that such is not the fact. There are molecular formulae and structural formulae. Which sort has Habermann in mind? As applied to molecular formulae, the chemical term homologous has a precise meaning. Two distinct molecular formulae are homologous if they both contain C and differ by a multiple of CH_2. Clearly, the term homologous here refers to a symetric and transitive relation. But it is difficult to get any two chemists to agree on a definition of homologous, if this term is applied to structural formulae or the single compounds to which such formulae frequently refer. Every chemist regards methane and ethane as homologous; scarcely anyone considers methyl alcohol to be a homologue of dimethyl ether. As to whether normal propyl-benzene is homologous with para-xylene, or whether benzil is homologous with dibenzoyl-methane, there

is wide divergence of opinion and usage. The only certainty is that the term homologous, as applied to structural formulae, refers to a relation which is symmetric but not always transitive. To apply exact reasoning to so vague a concept is to introduce into the subject a false and misleading sense of precision. If Habermann's conclusion is not to produce this result, its scope should be confined to molecular formulae.

As thus restricted, his dictum is easily acceptable, but again it comes close to being a truism. Every chemist knows that the general formula for a hydrocarbon is $C_nH_{2(n-x)}$ where both n and 2(n-x) are positive integers and $x \geq -1$. This statement follows directly from the facts that carbon is quadrivalent and hydrogen univalent. Hence, in order to obtain all molecular hydrocarbon formulae in linear order

(1) Let $x = -1$ and let n range from 1 to infinity.
(2) Let $x = 0$ and let n range from 2 to infinity.[2]
(3) Let x be successively each positive finite integer i, and in each case let n range from $i + 1$ to infinity.

As thus arranged, the entire set S of molecular hydrocarbon formulae has the ordinal number ω^2. It can be exhaustively and exclusively divided into subsets S_i (in the sense defined) and each such subset corresponds to a homologous series of molecular formulae.

Thus Habermann's imposing statements about hydrocarbons, if taken at face value, turn out to be for the most part unproven and probably unprovable. If charitably interpreted, they are almost self-evident consequences of the facts that:

(1) The valence of carbon is four and that of hydrogen one.
(2) Subject to this restriction, the general molecular formula for hydrocarbons is $C_nH_{2(n-x)}$ where both subscripts are finite and >0, but no other limit is placed on n.

If "Mengentheoretische Betrachtungsweisen" always lead in chemistry to such trivial results, the chemist may well dispense with their aid, but it is to be hoped that they have a brighter future in this field.

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[2] A slight irregularity is introduced into the early part of this series by the fact that CH_2 is not the formula for any hydrocarbon.

The Measurement of Time: A First Chapter of Physics

BY

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I. INTRODUCTION

AT A certain stage of advance in any science it may be well to re-examine and perhaps to rearrange its fundamentals. One seeks an ideal, logical order of development, which may or may not be the best pedagogical order. Many a high school beginner in physics has become acquainted with "force" as something that gets in between two bodies of matter and pulls them together or pushes them apart, like himself between two carts or like "magnetism" between two magnets. Whether or not this might ever be a proper view, the mature student of today will consider that for him, a knowledge of accelerations and strains, *inter alia*, is prerequisite to a knowledge of force.

In recent years the doctrine has been urged that physics should be established on observations rather than on intuitive notions. If, accordingly, one enters on the task of building up a knowledge of physics, what shall he observe first? Doubtless the best order for making the observations of physics is somewhat a matter of individual preference. The object of the present essay is to show that at an early stage of physics, if not at its very beginning, we may advantageously observe and count and compare sequences of like events. It is submitted that this procedure will involve

a minimum of intuitions and other prejudgments. To begin physics with consideration of sequences of like events is to begin with time relations in comparative disregard of space. Thus the concepts of time and metric time can be established with little or no attention to space concepts; whereupon the time concepts become available for building up the space concepts.

2. TWO SEQUENCES OF RECURRENT EVENTS WHICH YIELD A CONSTANT NUMBER RATIO

An ordinary gravity pendulum and a torsion pendulum are in motion before me. The gravity pendulum swings from side to side; the bob of the torsion pendulum oscillates, with its vertical steel suspension wire as axis. At first I notice that the gravity pendulum makes about four completions of cycle while the torsion pendulum goes from one completion of cycle to the next. With more care, presently I find that 82 completions of cycle of the gravity pendulum correspond as nearly as I can perceive with 21 completions of cycle for the torsion pendulum. Again and again I count the cycles and get this relation of 82 and 21; I leave them and start them again the next day and get this same relation, which I express concisely in the rational fraction 82/21 or in the equivalent ratio form 82:21. If I count any considerable number of the torsion pendulum cycles, I get a number of gravity pendulum cycles meanwhile that yields approximately the same ratio; thus if I count 35 cycles of the torsion pendulum I get, to a consecutive integer, 136 or 137 gravity pendulum cycles and the ratio 136:35 or 137:35, either of which is approximately equal to 82:21.

There are many different sequences of recurrent events which are related, one sequence to another, more or less precisely as for the pendulum cycles discussed in the foregoing paragraph. Such sequences are familiar in day-dawns, clock-ticks, winters, Sundays, pay-days, hair-cuts, shoe-shines, breakfasts, variable star cycles, breath cycles, drops from a leaky faucet, bell-strokes, steps of walking, heart-beats, summer solstices, new moons, etc., like events that happen again and again. Between any two series of such events there is a numerical relation like that for the cycles

of the two pendulums, more or less precise in dependence somewhat on how far the count is carried for each series. Generally the relation may be expressed by a rational fraction; when it is an integer it may be looked on as the special case of a rational fraction which has the denominator unity. Some such series defy comparison unless a rather large number of events in a series is taken, as for example the series of rain storms when compared with another series such as full moons.

There are certain other sequences which compare somewhat differently; these exhibit a precise one-to-one relation, as sunrises and sunsets, plantings and harvests, Sundays and Mondays, bell-strokes seen and bell-strokes heard. At places on the earth where there are two high tides in each approximate 25-hour period, we have the precise relation 2:1 for high tides and moon transits. The phenomena which yield these simple precise ratios, as 1:1 and 2:1, are in contrast with those considered before, such as in connection with the pendulums, for which the ratios are seldom simple and have only a degree of precision. It is easy to say that there is a kind of causal connection in the cases of sunrises and sunsets, tides and moon transits, which is not present in the case of gravity pendulum cycles and torsion pendulum cycles. In some of these cases with the simple, precise ratios, we are comparatively passive observers, as for new moons and full moons; in other cases we are active agents, as for pushes on a pump handle and spurts of water from the pump spout. Though there may be some excuse here to discuss the subject of cause and effect, this will not be necessary for our present purpose. Our special interest is in the event sequences like the pendulum cycles, which yield ratios that are usually neither simple nor precisely ascertainable. But we do not exclude from our view those cases which yield the simple precise ratios; therefore we do not need to distinguish them more definitely than we have done. All these events happen as they happen; our chosen task is to observe them and to make record of our observations in whatever comprehensive and concise description we may be able to discover.

Accordingly, we mark an important early stage of progress in the performance of that task when we say that *there are in the*

world many paired series of recurrent events such that between the two series of a pair there is obtained by counting a nearly (or exactly) constant ratio like the ratio 82:21 for the cycles of a certain gravity pendulum and the cycles of a certain torsion pendulum.

A brief digression will be proper for the remark that though the discovery of event sequences which are comparable by number ratios may come at an early stage in a study of physics, obviously it should be preceded by another important discovery of a general nature, namely the constancy of tallying, and the recognition of that constancy by naming the numbers and using the number-names in counting. In our present discussion we presuppose the "natural" numbers and the rational fractions based on them. However these numbers may be made to arise by processes of logic, it is probable that in actual human development they have become known by experience of tallying. A primitive shepherd who knows nothing of the names for the numbers may check his sheep in and out of their pen by passing pebbles from one hand to the other, one pebble for each sheep.

We might compare the pendulum cycles without reliance on number conventions, as by dropping pebbles in two cups, one pebble in one cup for each cycle of the gravity pendulum, and one pebble in another cup for each cycle of the torsion pendulum. Thus we should get 82 pebbles in one cup and 21 pebbles in the other cup, though we might be quite ignorant of these and all other named numbers. Then, again and again, after removing the pebbles and dropping them in their respective cups as before, we should find that the last pebble for the gravity pendulum cup and the last pebble for the torsion pendulum cup would go back together. All this could be done without knowledge of counting; but for the repetition of various experiments with various sequences of events, one would need a large shelf of cups of pebbles. The advantage of the important invention of naming the numbers and counting is obvious.

In all cases of tallying as well as of counting, the sequential elements compared are not material objects, but events of human experience. Each pendulum cycle completed is tallied not with a pebble in its place, but with a pebble shifted—more precisely,

with a shift of attention from one pebble to another. We begin our study of physics as a science of time events, not "space-time" events, not as a science of space-filling objects.

Though tallying and the number names and counting may properly come at an early stage in an orderly development of the fundamentals of physics, perhaps even earlier might come a study of the general subject of correspondences or one-to-one relations of events. Also, the important device of language should be considered, with its extensive and imperfect system of correspondence between memory images and words. However, we are not attempting here to find the lowest possible base on which to begin to build a knowledge of physics, but rather to find a base far enough down to be tolerably secure. Without further discussion of what could or should be the very beginning of physics, we begin here with counts of events; to this extent we disregard the familiar dictum that physics is a science of pointer readings.

3. LINKING THE RATIOS FOR MORE THAN TWO SEQUENCES; RATABILITY DEFINED

Starting with the gravity pendulum, I have found that its cycles compare with those of the torsion pendulum to give the ratio 82:21. I cause a little stream of water to flow under constant head; while the torsion pendulum completes 195 cycles, this stream fills a certain jar twice, giving the ratio 195:2. Again, while the jar fills 85 times, a certain style of candle burns 6 in succession, giving the ratio 85:6. And again, while 9 of these candles burn in succession, there are two successive cycles from sunrise to sunrise, giving the ratio 9:2. So we might go on linking the ratios from series to series. The relations here involved are so important that they are restated in the following table:

Gravity pendulum cycles completed	82			First ratio
Torsion pendulum cycles completed	21	195		Second ratio
Water jar fillings	2	85		Third ratio
Candles burned	6	9		Fourth ratio
Sunrise to sunrise cycles completed	2			

It will be seen that any one sequence links two ratios; thus the sequence of the candles burned has 6 candles in the third ratio

and 9 candles in the fourth ratio. Also, any one ratio links two sequences; thus the fourth ratio has 9 candles in the candle sequence and 2 sunrises in the sunrise sequence.

By a simple computation, each new series of events may be compared directly with the first series. Thus, (82:21) × (195:2) = 7,995:21, which is the ratio that compares gravity pendulum cycles with water jar fillings. This ratio multiplied by 85:6 gives 679,575:126, the ratio of comparison for gravity pendulum cycles and candles burned. Similarly, this multiplied by 9:2 gives 2,038,725:84, the ratio for gravity pendulum cycles and sunrise to sunrise cycles. Performing the last indicated division, there are about 24,270 of these gravity pendulum cycles in an ordinary twenty-four hour day.

It is conceivable that there might be several such systems of ratio-linked series in the universe; as if the revolutions of Saturn's moons were linked in constant ratios, and likewise for Jupiter's moons, but with no constancy of ratio between one of Saturn's moons and one of Jupiter's. In fact, if any one of Saturn's moons is compared with one of Jupiter's, the ratio is no less constant than for any two of Saturn's moons.

Observation teaches that *in all the accessible universe, various sequences of events are linked by well-defined, nearly (or exactly) constant ratios in only one extensive system.* So far as has been observed hitherto, such recurrent events as fit badly or not at all to the one system do not fit together in an important rival system.

An example of a trivial rival system will now be mentioned. I jack up one rear wheel of a certain old automobile, start the motor, and set the gears in "low." While a child plays with the throttle I observe certain rotating shafts which I designate A, B, C, D, E, in order from the engine to the spinning rear wheel. A turns 31 times while B turns 16 times, thus yielding the ratio 31:16 for A and B. Similarly, I get 29:18 for B and C; 71:16 for C and D; and 1:2 for D and E. These ratios may be multiplied together to obtain the ratio for the turns of the engine and the rear wheel, $\frac{31}{16}\cdot\frac{29}{18}\cdot\frac{71}{16}\cdot\frac{1}{2} = 6.924+$. Here are well-defined linked ratios; but if I count the turns of any one of the five shafts

against the ticks of a clock, I shall not be able to discover a constant ratio. The saving word in the foregoing italicized statement is "extensive." Instead of "one" system, there are at least two, namely, the universe and the automobile; but there is only "one extensive system," namely, the universe.[1a] That the ratios are precise for the automobile, but only more or less approximate for the universe, is not necessarily an important distinction. We might have a machine with belt drives where the automobile has spur gears, and our ratios would be no more precise than for the universe.

If you ask what is meant by the universe, the reply is—all the events of human experience. For the present, the only time order of these events to be considered is their order in the experience of one or another human being. To emphasize the distinction from any other system of time, the sequential order of events for a single human being will sometimes be called his egocentric time. A period of egocentric time consists of the sequence of events of a person's experience which happen after one event and before some other later event.

The one-to-one ratios present no serious difficulty. They link with the one extensive system of the universe, or they link only (if at all) in narrowly restricted systems. The one-to-one ratios for sunrises and sunsets, for clock ticks heard and second hand jumps seen, belong in the general system of the universe. On the other hand, the one-to-one ratios for lightning seen and thunder heard, for pressures on a hand numbering stamp and numbers printed, belong in respective restricted systems.

As the reader will have noticed, we are getting over the ground rapidly, with only brief mention and exemplification of salient

[1a]In a paper on "The Scientific Conception of the Measurement of Time," which was read before the Aristotelian Society on June 1, 1885 (published in Mind for July 1885, vol. 10, pp. 347 to 362, especially p. 355 *et seq.*), E. Hawksley Rhodes distinguished as "independent" and "dependent" these two classes of sequences of events which are here distinguished as "extensive" and "trivial." Inasmuch as his terminology seems to have been put to little or no use by others, I consider myself free not to employ it; a reason for not doing so in this instance is to avoid the implication of a causal relation which is carried by the word "dependent." In my opinion, if authors on the subject of time as a basic concept had given closer attention to this well-reasoned paper, sound thinking on that subject would have been greatly promoted during the past fifty years.

aspects of our subject. We enter upon no discussion of the delimitation of events, the decomposition of an event into simpler, elemental events, the profound distinction between the present event and past events, etc.[1] Our ratios have been rather crude approximations. To gain an idea of their possible error, the following routine could be proved and practiced. Call an event of the slower sequence the zeroth event, and count its events thereafter to any convenient number, say 100. Let n be the number of events of the faster sequence occurring within the period from the zeroth to the one hundredth event of the slower sequence. Take the ratio to be $(n + 1)/100$, with assurance that its absolute error will not exceed $1/100$, and its relative error will not exceed $1/(n + 1)$.

As examples of many precautions that would be conducive to greater precision, it may be mentioned that when gravity pendulums are employed, they should be operated at small and fairly constant amplitudes; and when candles are burned, the surrounding temperature should be kept fairly constant. With respect to Sundays and Mondays as examples of one-to-one ratios, the observer must not cross the date line in the Pacific Ocean. With respect to bell strokes seen and bell strokes heard, he must not vary his distance from the bell. And so on. There are methods and apparatus by which events may be counted easily; in some of these photoelectric cells are used. There are computation methods which are helpful in ascertaining the best fitting ratios; in some of these continued fractions play a part. Of these and other such ancillaries we shall say no more. Incommensurable ratios need not enter our system, for they arise from the artificial premises of pure mathematics and not from the observations of physics; they may be convenient in some cases, but they are not necessary.

We shall find it convenient to use the word "ratable" with special meaning as follows:

[1] Without pretense of annotating these allusions, a single reference will be given by way of example: See Bertrand Russell, On order in time, Cambridge Phil. Soc. Proc., Vol. 32 (May 1936), pages 216 to 228. This begins "Instants are mathematical constructions, not physical entities . . . "

(a) Two sequences of events are ratable when the ratio of the respective numbers obtained by counting them within a period of time is without exception nearly (or exactly) the same as when they are counted within each of several other periods of time.

(b) A single sequence of events is ratable when there is a considerable variety of other sequences with each of which it has the ratability of paragraph (a).

Ratability is generally a matter of degree, dependent on observations, and not determinable with absolute precision. As closely as I can observe, I find 82:21 as the correct constant ratio for the cycles of the gravity pendulum and the torsion pendulum heretofore considered, but if I were to try 21,000 cycles of the torsion pendulum, a departure from 82,000 for the number of the gravity pendulum cycles would probably become apparent; and this departure would be different at different seasons of the year, with different temperatures in the neighborhood, though differences of illumination would have no noticeable effect. At this early stage of our study of physics we exemplify the important principle that the conditions of experiments may often be adjusted empirically to bring the results more nearly within the terms of concise descriptive laws.

4. SELECTING A STANDARD REFERENCE SERIES OF EVENTS

In the foregoing section we made comparison of various series of events with a particular series, namely, the series of the gravity pendulum cycles. This suggests that we ought to select some suitable recurrent sequence of events as a standard of reference for all our affairs.

In making choice of a standard series of events, it is all too easy to hit on a series such that, when some other series is counted in comparison with it on different occasions, there is only an imperfect approximation to ratability as defined in paragraph (a) of the foregoing definition. Thunderstorms and animal heart beats are examples of series with which other series will not compare quite or almost precisely in constant ratios.

There are now known to us a considerable number of different series of events such that the ratio of any two of them is nearly

the same at different trials; nearly the same, but generally not exactly the same. The gravity pendulum and the torsion pendulum may give a slightly different ratio on a hot day and a cold day. At this stage of our study we are not prepared to attribute the discrepancy to either of these pendulums in particular, but if we were to try them on hot days and cold days against a multitude of other series of events, we should learn that the gravity pendulum gives more nearly constant ratios than the torsion pendulum.

During many centuries men have tried various different series of events as standards—candle burnings, shadow transits (by sun dials), moon phase cycles, water jar fillings (by clepsydra), sand vessel emptyings (by hour glasses). In the last few centuries we have settled on two series of events, each preeminent for its ratability (as defined in paragraph (b) of the foregoing definition) and its convenience of observation. These two series are (1) meridian transits of a fixed star (marking earth turns), and (2) completions of cycle of a gravity pendulum, specially constructed and kept at a constant temperature and otherwise subjected to special care. Such pendulums are in operation at the leading government observatories, as in Washington, Greenwich, and Paris.

Consider an astronomer in such an observatory. On two successive nights he observes and records the meridian transit of a certain fixed star. In his egocentric time the sequence of events of his conscious life between those two transits is one sidereal day, and between two other such transits is another sidereal day. The dependence of the second on the gravity pendulum and its relation to the sidereal day will be explained in the following section.

We cannot say that two sidereal days are of equal duration until we determine what that would mean. We cannot place them side by side as we naturally would do for two yard sticks. Nor can we carry a measure from one to the other back and forth, as we might carry a pair of dividers back and forth between one line segment and another to discover whether they are equal. Baffled in any such effort, we turn about and lay down the defi-

nition: *In a ratable series of events, let it be said that the time intervals or durations between any two consecutive events are equal.* Since star transits are a ratable series of events, therefore under this definition sidereal days are equal. *When we say two periods of time are equal, we mean that they are marked by the same number of events of a ratable series.*

Apart from such definition, is it possible to discuss the question whether two sidereal days are "really" equal? Or whether one and another year of such days are "really" equal? Is it possible that the days and the year of 1885 were really twice as long as the days and the year of 1935? To an old man a year of his youth may seem longer than the year before last, but for purposes of scientific description, we dismiss his impression as "subjective." On the basis of tangible observations, the first question of this paragraph must be answered in the negative.

5. DERIVED AND SUBMULTIPLE UNITS OF TIME

At any one place on the earth's surface and for any one person, the period of a solar day may be taken as comprising the sequence of all the events of his conscious existence from one meridian transit of the sun to the next. The sequence of solar transits is not ratable to a high degree. The discrepancy is considerable, as is well understood in connection with the so-called "equation of time." But the sequence of fixed star transits, marking sidereal days, has been found to be ratable in high degree.

To make a long story short, as a result of much careful study and accumulated knowledge, astronomers have formulated the rule that they will seek and establish a series of events, ratable with the star transit series, such that their ratio shall be $(1{,}000 \times 24 \times 60 \times 60 - 235{,}909)$ of these events to 1,000 of the star transits.[2] The beats (half cycles) of a certain gravity pendulum about 39.14 inches long give such a series of ratable events, and the name "second" (the second of mean solar time) is conferred on the totality of events of any one person's egocentric time between a definite phase or aspect of one such beat

[2] H. N. Russell et al., Astronomy, Ginn & Co., Boston, 1926, Vol. I, Appendix, Table III.

witnessed by him and the like phase or aspect of the next beat which must also be witnessed by him. *This unit*, so derived from the sidereal day, *is the principal time unit of physics.*

Of these unit seconds of time, 60 in sequence are defined as making a minute, 60 × 60 of them make an hour, and 24 × 60 × 60 of them make a *mean* solar day. Indeed, the numbers in the ratio in the astronomers' rule mentioned above were chosen to make the sidereal day have the proper ratio with the mean solar day. The sidereal day is 235.909 mean solar day seconds shorter than the mean solar day. If yet smaller units than the second are required, a tuning fork is constructed and adjusted so that it makes say 1,000 vibrations while the seconds gravity pendulum goes from a certain definite phase of one beat to the like phase of the next beat. In recent years it has been determined that the vibrations of a crystal in an oscillatory electric circuit are highly ratable. These afford a sequence of recurrent events of high frequency.

We think of all the foregoing determination of a primary unit of time (sidereal day) and the derived units (second, hour, thousandth of a second, etc.) as having taken place in the observatory and attached laboratory of the astronomer. Though the observed star is a long way from the astronomer, the event which he observes as its transit, occurs at or near the time when the light enters his eye, not when that light left the star, whenever that may have been. The whole process is an affair of his egocentric time.

This determination of a time measuring system may be summarized thus: Of a vast number of different series of recurrent events, each series being more or less ratable, we select as a primary standard a series (star transits) having superior ratability, and define that the ensemble of events of an egocentric time system between two consecutive events of the chosen series is a standard primary time unit (the sidereal day). For a smaller derived unit such as the second, we provide apparatus (a gravity pendulum) and adjust it so that it will perform the proper number of ratable recurrent events (like aspects of successive beats in the

case of the gravity pendulum) to give the desired ratio with the chosen primary standard.

Having developed this time measuring system, let us consider an example of its use. Suppose our astronomer wishes to measure the duration of an eclipse. He may watch his seconds pendulum and count its beats from the beginning to the end of the eclipse, but he finds it more convenient to provide an automatic counter, a clock. If he desires to measure the time period more precisely, he may employ an electrically operated automatic counter in connection with his tuning fork, and get the time period in thousandths of a second.

In all this, if we seek to compare one second with another second, or one thousandth of a second with another thousandth of a second, there is no other equality than as expressed in the italicized words of Section 4.

Notice how early in a study of physics we are able to compass the measurement of time. Equipped with the system of the natural numbers and the rational fractions, we have proceeded directly and in order to the discovery of constant ratios of comparison of paired sequences of recurrent events; the discovery of the linkage of these ratios in an extensive world system; the choice of one series (sidereal days) as a standard for measurement; and the choice of other related series to yield convenient units for all time measurements. To have attained this stage so readily is significant of the fundamental nature of time and its place in the foundations of physics. Similarly to compass the measurement of space would be a more difficult task.

6. EXTENDING A TIME SYSTEM BEYOND THE WALLS OF THE LABORATORY

When Galileo Galilei made his pioneer observations of the great chandelier at Pisa swinging as a pendulum, he timed it by counting his own pulse. When he made his famous experiments with falling bodies, especially bodies falling along inclined planes, he had no clock adjusted to keep step with the stars. Before an experiment he held his finger tip over a small orifice in the wall

of a large vessel of water; at the start he opened it and at the end he closed it by means of his finger, catching in a cup the water that flowed meanwhile, and weighing it to get the elapsed time. His example illustrates the fact that for a beginning study of physics, we do not need to extend our time system beyond the walls of our laboratory. Nevertheless it will be proper here to indicate very briefly how such extension may be accomplished.

It requires an appreciable lapse of time to send a signal to a far distant place on the earth's surface and get an answer back. From Washington an electric current pulse can be sent by wire through amplifiers to San Francisco, there amplified and reflected, and returned through the same wire and amplifiers to Washington, where it will be received a fraction of a second after it was sent. Call this period of time t, measured in thousandths of a second. If the signal is sent $\frac{1}{2}t$ thousandths of a second before the hour by the Washington clock, the reflected signal will be received there $\frac{1}{2}t$ thousandths of a second after the hour by that clock. Let the clock at San Francisco be set on the hour when the signal is reflected there. We must not assume without basis that the signal takes the same time to go one way as the other. To test this question we need to have the clocks set together at both places. Thus we are in a circular difficulty. We need to know that the to and fro periods are equal in order to set the clocks together, and we need to know that the clocks are together in order to test whether the to and fro periods are equal. Part of our debt to Einstein is that he has taught us that the question whether the clocks are together is not meaningful unless and until we can set up an operative test to establish their relation in this respect. We leave this difficulty of simultaneity at distant places with the remark that it has been extensively discussed in recent years in connection with Einstein's theory of relative unaccelerated motion.

In ways such as we have described for Washington and San Francisco, by electric signals, wire and wireless, by observations of the same star at different places, etc., the clocks of the world have been set, we do not now say together, but in a very satisfactory kind of correspondence. This world system works and

checks as harmonious in many ways. By the aid of this harmonious time system, an event anywhere on the surface of the earth may be fitted to a particular egocentric time system. That there is an arbitrary element in this harmonious time system should not be overlooked, as in the effort to set the San Francisco clock with that at Washington.

No universal time system exists in its own right, but only as a construct built up from egocentric time systems.

7. ABSOLUTE TIME

It is evident that there are as many egocentric time systems as there are individual persons. Your time is marked by your succession of events of consciousness, and in general these will be different from the succession in my time. But when we are together and compare notes, I perceive that according to your conduct, your sequence corresponds in great measure with mine. You seem to see and hear what I see and hear, and in the same order. Certain cases that appear exceptional are easily reconciled. You and Smith and Jones and I stand in a row at intervals of a thousand feet. You raise your hand and at sight of it Smith and Jones strike bells of distinctive tones. You hear Smith, then Jones; but I hear Jones, then Smith. We reconcile the discrepancy by the well tested theory that it takes longer for the sound to travel two intervals than one.

In such manner in support gained for the intuition of a universal time by which in some way our individual time systems are controlled. This is something conjectured, not observed. The utmost time system that is observed directly by any one of us is his one egocentric time system. It may also be observed that this corresponds extensively with the reported systems of one's neighbors. If this seems to imply a dependence of all these time systems on a more universal system, it must be remembered that no such dependence is proved; it is not observed, nor can it be deduced from observations; it is an intuition, employed as a working hypothesis, not of necessity, but for convenience. Most studies of physics are affairs of egocentric time, made by single observers. When several observers are involved in a study, it is carried for-

ward from the standpoint of a single observer. His assistants are machines to him, indicating results by words instead of on dials, operated by words instead of by pulling levers and pushing buttons.

If one were looking at a printing press and should notice two wheels turning in simple definite ratio, as 13:5 for example, he would suspect that with further observation he would find them both driven by gearing from the same shaft. There are persons who look on the universe as like the printing press. They feel, if they do not believe, that there is a great invisible, frictionless master flywheel which they call "Absolute time," and that the visible mechanism of the universe, the planets in their orbits and the hands of the watch in one's pocket, are all geared invisibly into this Absolute time flywheel. In the case of a spinning top which dies down as it spins, there must be a slip connection somewhere between it and the master flywheel, or perhaps there is a spiral cam gear. This intuition of Absolute time may or may not be helpful as a working hypothesis; but if we hold our imaginations in check, we must agree that the master flywheel is nonexistent. What has been observed to exist is that in any one egocentric time system many paired series of events are linked in a single extensive system of ratability, and among various such egocentric time systems there is an extensive correspondence.

The persons who hold to the Absolute time intuition usually consider that its master flywheel turns uniformly. But if there were a standard by which to demonstrate its uniformity, then to be consistent they must look upon the master flywheel of Absolute time as being geared into that standard, which thereby becomes its master, a *reductio ad absurdum*. Sometimes it is said that Absolute time must be uniform with itself. But no past hour can be lifted from the past and placed alongside the present hour to demonstrate such a uniformity. To say that Absolute time is uniform with itself is an empty form of words unless and until the uniformity can be observed; no such observation is possible.

If it occurs to you to regard the events of a ratable series as like equally spaced points along a line, that puts the measurement

of space before the measurement of time. But this whole subject of time measurement can be developed and determined with little or no recognition of the concept of space; an ideal for which we have striven. Another thought may be to regard a ratable series of events as marking a motion with uniform velocity. But velocity is distance divided by time, and distance is a space character which is quite unnecessary for the measurement of time. When we enter on a study of the foundations of physics, we have only three choices in respect to time and space; we may recognize first one, or the other, or both. It is comparatively easy to study time apart from space, but difficult, if not impossible, to study space without the aid of the ideas of time. It helps to an understanding of space to regard its phenomenal manifestations as events of time. Since there is a rather obvious probable gain of simplicity if we take on new topics singly, it seems better to study time before space. But the principal argument for or against this plan must be the degree of success attainable by pursuing it.

For the view that there is a super-system of time, the authority of Newton is sometimes invoked: "Absolute, true, and mathematical time, of itself and from its own nature, flows equably without relation to anything external. . . ."[3] It seems reasonable to suppose that this was meant by Newton as a mere hypothesis of convenience, with emphasis on the word "mathematical"; but if it is to be taken as an assertion of physical fact, we are not willing to let the matter be settled for us by an appealto authority. Yielding to no one in recognition of Newton's genius and candor, we cannot close our eyes to the fact that sometimes he erred, as for example in his belief that an achromatic lens would be an impossibility.[4] Newton's Absolute time is no doubt suitable within limits as a working hypothesis, a convenience of thought under some circumstances, but quite without any direct observational basis of fact.

[3] Isaac Newton, Principia (Motte's translation, revised by F. Cajori), U. of Calif. Press, 1934, Book I, Scholium to Definitions, page 6.

[4] Isaac Newton, Optics, G. Bell & Sons, London 1931, page 102 and context, also page xvii of Introduction by E. T. Whitaker.

8. A SENSE OR SENSATION OF TIME

Continuity and discreteness are antithetical. Their irreconcilability is at the bottom of Zeno's paradoxes. No one has succeeded in banishing discreteness from scientific descriptions, but we have many forcible reminders of the banishment of continuity. One sees a smooth red wall, but as he walks toward it, he faces a pile of bricks. One hears a pure smooth tone from a flute, but it is well known that what comes from the flute is a sequence of puffs of air. Modern quantum theory carries forward the description of nature in terms of discrete elements. Is our immediate perception of the passage of time based on a direct apprehension of continuity or is it an awareness of successive events?

To test the view that we apprehend time as a continuum, one must suppress all bodily activity, as by lying quietly in bed; next, he must shut himself off from sensations. One can scarcely look fixedly at a scene of some complexity without shifting attention from one feature to another; each such shift is an event. Unless great care is exercised, there will be sounds to reach the ears. But shutting off activity and sensation as far as possible, nevertheless the heart goes on beating, and breathing cannot be halted for long. These and other physiologic processes undoubtedly contribute to give us a notion of the flight of time. Though activity and ordinary sensation are inhibited as far as possible, there are our thoughts. These are events. An unvarying continuum of thought is no thought at all. Many psychologists consider that thoughts are always accompanied by incipient operation of the muscles and nerves that function in normal speech, if not in other bodily activities. In some cases, this is rather apparent, as when in repose I count or run through the alphabet "mentally." If, as William James suggests,[5] I keep my mouth open and try to get a vivid image of such a word as "bubble" or "toddle," it is unsatisfactory and I am conscious of the impulse to close the lips or to touch the tongue to the palate.

Thus, whenever we are conscious, we are dragged along by

[5] Principles of Psychology, Holt, N. Y. 1890, Vol. II, pages 63, 64.

recurrent mental events, thoughts as well as sensations; to a degree at least, it is our experience of these that gives us our primitive sense of the flight of time.

If we may invoke the hypothesis of the subconscious mind, so much relied on by many psychologists, perhaps our sense of the flight of time is merely a sensation of events in sequence, both conscious events and subconscious events. If you are one of those rare persons who can estimate periods of time accurately without conscious notice of passing events, perhaps your subconscious ego notices your breathing or heart beats, and an inference from these is all that rises through the dividing plane that separates the subconscious from the conscious realm. Such reliance on supposed subconscious observation of events does no more violence to the principles of a strict observationist than to suppose that you have a direct apprehension of Absolute time. It may be that when one wakes on a dark winter morning with the notion that it is time to rise, he is influenced by a feeling of rest gained in the night, or it may be that his subconscious *alter ego* has been estimating his breaths and heart beats during the night and reports the result to the conscious partner. On the latter view, when Rip Van Winkle lost twenty years, not only was he asleep, but his subconscious ego also slept.

Fever patients are said to guess at counting seconds at a faster rate than normal persons.[6] As determined by the temperature, an insect or other cold-blooded animal can be made to live fast or slow. The speed of all insect movement is quickened rather definitely with increase of temperature. At low temperature a parent fruit fly (Drosophila melanogaster) can be kept alive while successive generations of its offspring are born and live their lives and breed and die.[7] I doubt if the parent fruit fly feels older than its offspring. I suspect its feeling of age is not according to the sidereal days and pendulum beats, but is more according to the integrated activity of its life. A long life of

[6] Hudson Hoagland, The Chemistry of Time, Philosophy of Science, 1934, Vol. I, pages 351–2.

[7] Wells, Huxley & Wells, Science of Life, Doubleday, Doran & Co., N. Y., 1931, page 542.

sluggish movement seems no longer than a brief life of normal activity.

Until some evidence is adduced otherwise, the view seems proper that our sense of time has no other basis than the observation (sometimes dim and soon forgotten) of certain events of consciousness or subconsciousness that to a degree are recurrent and ratable. But whatever may be the source of this sense, its unaided exercise is so untrustworthy that we can make little or no use of it in physics. If there is a smooth flowing Absolute time that we sense directly, then we sense it very crudely. A cheap watch or clock will serve far better.

9. FICTITIOUS EVENTS IN SEQUENCE

Let us consider another instructive comparison of two sequences of events. I set two bodies in rotation; one is a massive flywheel in rather smooth bearings; the other is a stone disk floating in a basin of mercury. It is arranged that, as often as desired, both bodies can be started in rotation together, each receiving the same impulse as before. The disk carries a perforated flange which acts as a shutter, flashing a narrow beam of light about a hundred times in each rotation.[8] I observe as recurrent events the turns of the flywheel and the flashes of the disk. Very soon I learn that the two sequences of events are not ratable; this is shown in the first three columns of the table on opposite page.

Enters on the scene a competent mechanical engineer who plays the part of a wizard. Says this engineer-wizard: "I will give you two explicit functional formulas, one for the flywheel and one for the disk. If you will put your counts of events

[8] Of course the perforations in the flange should be equally spaced. If this seems to violate our principle that space-metric considerations should be postponed, there are two replies: (1) To refuse consideration of this experiment at this stage would be like the case of the student who refused to wear his glasses when he began the study of mechanics, on the ground that he had as yet made no study of optics. The instructor may make the flange with its equally spaced perforations part of the "given", and the student need have no concern as to how the perforations are distributed. (2) For the purposes of this experiment, general space-metric considerations could be evaded by directing the student to center the flange on a flywheel, to put enough torque on the flywheel to keep it at 501 rotations per 500 seconds, and to mark a place for a perforation on the flange at every fifth second.

into these respective formulas, they will give you numbers in constant ratio." Proceeding accordingly, I get the last three columns of the table, to which a few notes (from the point of view of the engineer) are appended for the reader who cares to consider the matter more thoroughly. Stated summarily; successive values of E_o are established by adding in its successive

Table for two observed sequences of events which are not ratable, though the corresponding sequences of fictitious events computed therefrom are ratable

I	II	III	IV	V	VI
Fly wheel turns counted in sets of 100	Corresponding disk flashes counted	Ratios	Fictitious events (See notes below)		Ratios
			Flywheel	Disk	
ΔE_o	Δe_o	$\Delta e_o/\Delta E_o$	$k\cdot\Delta E_f$	$cc'\cdot\Delta e_f$	$\frac{cc'}{k}\cdot\frac{\Delta e_f}{\Delta E_f}$
100	183	1.83	0.0550	0.2297	4.18
100	160	1.60	0.0584	0.2437	4.17
100	144	1.44	0.0626	0.2607	4.17
100	133	1.33	0.0677	0.2812	4.15
100	127	1.27	0.0744	0.3109	4.18
100	123	1.23	0.0836	0.3468	4.15
100	125	1.25	0.0973	0.4055	4.17
100	135	1.35	0.1215	0.5073	4.17
100	174	1.74	0.1871	0.7791	4.16

Note for column IV. These values are computed by the formula

$$kE_f = k'(1 - \sqrt{1 - 2kE_o/k'}),$$

which is obtainable by integration from $d^2E_o/dE_f^2 = -k$. From column I, $E_o = \Sigma\Delta E_o$, i.e. $E_o = 100, 200, 300$, etc.; k' (a constant of integration) is the initial velocity of the flywheel in turns per unit time; for convenience, the unit of time is taken to be such that $k' = 1$; k is the constant negative acceleration, which in this case is $k = 0.000535$ unit. From the values of kE_f, their differences are found and entered in column IV.

Note for column V. These values are computed by the formula $cc'e_f = e^{ce_o} - 1$, which is obtainable by integration from $d^2e_o/de_f^2 = -c(de_o/de_f)^2$. From column II, $e_o = \Sigma\Delta e_o$, i.e. $e = 187, 343, 487$, etc.; c' (a constant of integration) is the initial frequency of the disk flashes; in this case and in terms of the time units of column IV, $c' = 1.97345$; c is the negative acceleration at unit velocity, which in this case is 0.00113 unit. From the values of $cc'e_f$, their differences are found and entered in column V.

increments in column I; in the formula for the flywheel (given in the note for column IV), E_o is the number of observed events, flywheel turns, from a datum event, and E_f is a functionally dependent number, a number of fictitious events from the same datum; the corresponding increments of E_f are in column IV. Similarly for e_o and e_f in the formula for the disk flashes (given

in the note for column V). Though the ratio $\Delta e_o : \Delta E_o$ in column III is quite inconstant, the ratio $\Delta e_f : \Delta E_f$ in column VI is fairly constant.

The engineer's secret is this: For a body subject to a constant frictional retardation, like the flywheel, he knows the conventional descriptive law of its motion as a function of time measured in any convenient units. He turns this law about and gives it to me in the shape of a formula for the time in his chosen units as a function of the counted flywheel turns. Similarly, he turns about the law of motion under fluid resistance as for the spinning disk—here the resistance varies as the square of the velocity—and gives me this law in the shape of a formula for the time in his same chosen units as a function of the counted disk flashes. In each case, what I get from the formula is a number of time units, E_f for the flywheel and e_f for the disk; the fictitious events represented by these numbers are almost as ratable as the ticks of two clocks.

If all the sequences of recurrent events of our direct experience were like the flywheel turns and the disk flashes in their lack of direct ratability, there would be no reason for anyone to suppose that fictitious numbers determined in comparatively simple formulas by the counts of observed events would show ratability. If, nevertheless, there were such a hidden ratability, we should probably be very slow to discover it. Happily for us in our search for concise expression of knowledge, nature thrusts on our attention many instances of direct ratability. The engineer had such knowledge of these and other data that he was able to work back and uncover the hidden ratability, the invariant number ratio of the fictitious events, as determined by the motions of the flywheel and the disk.

Scientific research is in great measure a search for hidden invariants. Men perceived the inconstancy of velocity of falling bodies, but waited thousands of years for Galileo Galilei to perceive the constancy of their acceleration. It was after his day that men first perceived the invariance of kinetic energy ($\Sigma \frac{1}{2} mv^2$) in certain cases; and yet nearer to our day when Helmholtz and others made the important discovery of the extensive invariance of total

energy, kinetic and potential ($\Sigma\frac{1}{2}mv^2 + \Sigma\int Fds$), which leads to the law of conservation of energy.

If we had no direct experience of ratability, the discovery of the two formulas furnished to us by the engineer would be a great discovery, ranking with the discovery of the law of conservation of energy. Under a régime of no direct experience of ratability, we should have no reason to suspect a hidden invariant in the form a constant ratio of sequences of fictitious events functionally dependent on observed events. Only if there are constant ratios for sequences of events, observed or fictitious (but determined by observed events), can there be any measurement of time. In fact, measurable time, or ratability, is not a hidden invariant; it is a conspicuously apparent invariant.

All through physical science we encounter fictions. In optics, with a light source in front of a mirror, it may be more convenient to forget the mirror and have regard to a fictitious added source, the image. For another example, in developing the theory of electromagnetism and light for a body in motion, Lorentz assumed a normal time system for bodies at rest, and then he found his formulas much simplified by assuming for the moving body a related fictitious time system which he called the local time.[9] His doctrine was later comprehended within Einstein's theory of relativity of unaccelerated motion.

Whatever has been said here of a time system based on fictitious events, there is nothing fictitious in the definite succession of the events of your or my egocentric time.

Poincaré has said: "Time should be so defined that the equations of mechanics may be as simple as possible. In other words, there is not one way of measuring time more true than another; that which is generally adopted is only more *convenient.*"[10] It may be that in some degree the physicists of our day are in like case with an observer to whom all sequences of recurrent events are non-ratable, as for the flywheel turns and the disk flashes. There may be a series of fictitious events deducible by formula

[9] H. A. Lorentz, Theory of Electrons, Teubner, Leipzig, 1916, page 197 and context.

[10] Henri Poincaré, Foundations of Science, Science Press, New York, 1921, pages 227, 228.

and applicable in comparison with the count of the meridian star transits, such that the fictitious events would deviate definitely from the observed events (star transits) and form a time system better suited to a precise expression of the laws of physics than the time system of the star transits.

This possibility has been given serious consideration. We notice the tides and query whether an ideal of consistency should constrain us to infer a frictional retardation of the earth in its rotation, that is to infer a functionally dependent fictitious series of events which would be better for a standard time system than the actual earth turns in relation to a fixed star. The frictional effect of the tides is complicated by various factors and can be computed with only extremely rough approximation, but that ideal of consistency impels us to ascertain a measure of this effect in some tolerable degree of approximation. Thus it has been found that in a century, tidal friction retards the earth's rotation enough to lengthen the sidereal day by a period of the order of a thousandth of a second. The cumulative lengthening of the day varies as the square of the time; in a century it sets the earth back about 18 seconds; in 70 centuries the earth loses one complete day. Though the discrepancy is so slight, astronomers are confident that they have detected it in the records of ancient eclipses.[11] If they figure back in the ordinary way they get these eclipses as having occurred slightly later than when they were recorded. But their computations are in terms of fictitious earth turns, as if there had been no tidal effect; the results must be "corrected" to be expressed in terms of the actual earth turns of the records. Here is indeed a case of preference for a series of fictitious events to give a basic standard time series.

In comparatively recent years, not necessarily going back to ancient eclipses, there has been an observable discrepancy in the earth's motion (as noticed by star transits) and the moon's motion that has given astronomers much concern. For a long time it was mentioned as the moon's irregularity, with the tacit implication of the earth's regularity. Sometimes the moon has been

[11] H. N. Russell et al., Astronomy, Ginn & Co., Boston, 1926, Vol. I, pages 116, 117, 288, 289, 300.

observed to be as much as twenty seconds of time ahead or behind in its schedule as compared with observations made several years earlier or later. Actually, the observed irregularity is in the *relation* of the earth and moon; no observation can attach the irregularity to one more than the other. When the moon is ahead, it is ahead compared to the earth, and equally the earth is behind compared to the moon. However, it may be that our inductive generalizations and our formulas will become simpler by ascribing the irregularity to one of them than to the other; on this basis it is now supposed that it is the earth that is irregular, sometimes ahead of the schedule determined by the moon as much as ten seconds, sometimes as much behind, which makes twenty seconds between the extremes. So the astronomer conceives a fictitious series of events that may be ahead of or behind the real earth turns sometimes as much as ten seconds; his formulas for the moon's motion are far simpler in relation to the fictitious events than in relation to the real earth turns.[12] Another reason, perhaps somewhat *ad hoc*, why the astronomer finds it easier to ascribe the irregularity to the earth than to the moon is that he can suppose a pulsation in the size of the earth that would vary its angular velocity more easily than he can suppose any aberrancy that would vary the moon's orbital velocity. Though the gradient of this irregularity is at times far greater than that for the tidal effect, it is probably too small to be detected by comparison of terrestrial events.

10. THE IMPORTANCE OF THE RATABILITY OF SEQUENCES OF EVENTS

It may be an intuition with some persons that there must be a high degree of ratability for various different series of recurrent events, but obviously we all have experience of many sets of recurrent events that are ratable to a low degree only, or quite non-ratable; such as heart beats, hail storms, cock-crowings, passing automobiles, volcano eruptions. Making exception of the cases of simple precise ratios as for sunrises and sunsets and of such trivial cases as for the automobile discussed in Section 3,

[12] H. N. Russell et al., Astronomy, Ginn & Co., Boston, 1926, Vol. I, pages 117, 289, 290.

it is easy to see that this might be a quite livable world, even if there were no readily discoverable general system of ratability among sequences of recurrent events.

In such a world we could select a particular series of events as standard and define that its time intervals or durations are equal, but it would be a vain thing to do. On one day the scientist might observe closely the related numbers of comparison with some other set of events, but the next day the comparison could not be expected to give numbers having the same ratio. In such a world of non-ratability or low ratability there might be abundant opportunity for science, but in this science the exact measurement of time could not be expected to find a place.

In a world of apparent low ratability, it might be that events of high frequency, say vibrations of various tuning forks, would be ratable within short overall periods of observation, a few minutes or hours, but would be quite non-ratable for longer periods, a few months or years. It may be that in the world as we find it, our observed ratabilities are transient. The moon's irregularity would never have been established within a single year of observations. A thousand years hence, who knows but that such variety of "irregularities" will have piled up on the astronomers as to make hopeless the task of reconciling them?

The reader is urged to let his thought linger a minute on this conception of a world of non-ratability of sequences of events, a world in which there could be no such thing as measurement of time. Among the reasons for this suggestion is that it should have heuristic value toward gaining correct ideas of space. In an effort to discover what is the essential basis of space measurement, one should consider a space so simple and elementary as to be incapable of measurement; the space of topology.[13] For this, the necessary mental attitude will be easier to one who has become accustomed to the idea of a non-metric time. Knowledge of space builds on knowledge of time. With John Stuart Mill's characterization of matter as a permanent possibility of sensa-

[13] See the paper by Philip Franklin, "What is topology?", in Philosophy of Science, Vol. II (1935), pages 39 to 47.

tion,[14] a sensible line of points is a recurrent sequence of conscious events. Alfred North Whitehead says that geometry is the science of cross classification of points.[15] According to these views, each sensible event which we call the perception of a physical point is identifiable as occurring in each of several different sequences of recurrent events. The discovery of this cross classification is the discovery of space; not intuitive space (whatever that may be), but observable space, an observable relation in which the relata are certain events of human experience. A practicable and proper development is to take the time order *sui generis*, and then proceed to describe space in terms of those permanently possible elemental sensation events (physical points) which we find to be cross-classifiable in a plurality of different sequences.

Either time or space may be non-metric without being disorderly. Recitals of the alphabet in non-metric time may always bring forth *a* before *e* and *e* before *i*, though it would be somewhat if not quite meaningless to discuss whether the duration from *a* to *e* equals the duration from *e* to *i*. In such a time system, the effort to measure any period between events would be like trying to measure the interval between passing automobiles by counting the crow calls from the neighboring woods.

The fact of a single universal system of ratability seems to me to be of profound fundamental importance. With no observable connection except that the observer is one and the same person, all these different sequences of events, grand and small, are occurring at their appointed rates. The chronometer of the sailing ship affords a wonderful example, especially going back to the days before its importance had been diminished by radio signalling; its balance wheel holding its precious rate so nearly constant from port to port around the world, for long weeks the ship master's best clue to his longitude on the wide ocean. Why should its rate keep so nearly constant with the star transits

[14] An Examination of Sir William Hamilton's Philosophy . . . , 6th ed., Longmans, 1889, page 233 and context.

[15] The Axioms of Projective Geometry, Cambridge Tract in Mathematics and Mathematical Physics, No. 4, 1913, page 5.

observed at Greenwich? Why does Delta Cephei which is millions of times as far away as the moon always wax and wane in brightness about eleven times while the moon goes twice through its cycle of phases? Why does the pendulum of the cuckoo clock make about 111 beats while the hall clock pendulum beats 60 times?

To answer such questions by saying that the constant ratios are consequences of certain laws of physics is merely to state observed facts more comprehensively. Suppose you enter a great banquet hall alone and see a thousand cups on the tables and you look down in one of them and see a collar button, then in another cup you find a collar button and likewise in each of a hundred cups. Then another person enters the hall and looks in one cup and says "Here is a collar button; why is that?" and you reply, "For the reason that in each and every cup there is a collar button." You are not giving a reason in any other sense than stating an inferred generalization of what you have observed.

The laws of physics are generalized inductions from observed uniformities of nature, or are deduced from such inductions. Before there can be any law of physics whatever, some men must observe and observe. Eventually they will make a single descriptive statement to embrace a multitude of these observations. This will be a law of science. From it and other such laws, perhaps yet further laws may be deduced. The possibility of these laws lies in the uniformities of observed nature, and the laws are descriptions of those uniformities. Single examples are not *consequences* of the laws but mere instances of the multitude of cases in description of which the laws were framed. Implicitly if not explicitly, indirectly if not directly, ratability is established by observations; its mystery is not dispelled by turning about and saying that it is a consequence of the very laws that were framed as its description.

Another type of reply to the foregoing questions is exemplified by saying that the pendulums of the cuckoo clock and the hall clock beat 111 to 60 because it is a law of physics that with a certain acceleration due to gravity, the number of beats of any simple pendulum in a given period of time will be inversely pro-

portional to the square root of its length; that if I will measure these pendulums, I will doubtless find their lengths to be in about the ratio of $\sqrt{60}$ and $\sqrt{111}$; and that is the reason why their beats count again and again 111 against 60. But it seems to me that this attempts the difficult feat of building our structure of knowledge from the top down. This professed explanation presupposes a full discovery of the metric properties of space, and the further discovery of time events having space-time metric relations. My plan is to discover such time events at the outset.

Doubtless, observations of physics can be made and generalized into laws so that cases of ratability will appear as deductions therefrom; but to make such observations will be more difficult and complicated than to observe cases of ratability *per se*. Before we can relate the regularities of nature, we must observe them; and before we specialize on physics, we shall have established a knowledge of elementary number theory. If, now, we begin to gather the special data of physics, the most tangible subject matter for our observations, uncontaminated by intuitions, is to be found in sequences of recurrent events. Their ratability is either a matter of immediate observation, or of deduction from generalizations about other observations. The first alternative is simple and direct; the second is complicated, indirect, and much affected with intuitions.

Directive Action and Life

BY

RALPH S. LILLIE

WHEN we consider closely any highly integrated vital process, like embryonic development, or animal behavior of the end-subserving or purposive type, we are inevitably impressed with the importance of those special controlling factors, collectively termed "regulative," which appear chiefly responsible for the unified and finalistic character of the whole sequence of events. These factors are persistent in their influence although they may act intermittently.[1] Without their presence the sequence would soon lose coördination and "run wild," just as an automobile runs off the road unless occasional compensating touches are given to the steering wheel. They are the factors which give unity and direction to the total process. Generalizing, we may say that in any complex unified process, especially a synthetic one—where the progress is from less to greater complexity—integration seems always to imply or presuppose the operation of regulative factors having the character which we call *directive*. These keep the sequence of events

[1] Driesch points out that an entelechy need assert its control only intermittently, relying in the intervals upon the automatic physico-chemical processes of the organism (*Cf. e.g. Science and Philosophy of the Organism*, 1st Ed., Vol. 2, p. 237). Here the entelechy is coping with conditions like those which beset any kind of purposive activity, where, as every observant person is aware, a main part of the controlling action consists in compensating or correcting for casual or random events. Typically the controlling agent lets events take their course and interferes directively only at certain selected places and times.

to a definite course which in the living organism usually terminates in some outcome having special biological significance, or survival value. Apparently the directive factors always exist side by side with randomly acting or non-directive factors; but it is clear that a preponderance of uncompensated randomness would be inconsistent with the special character of the biological process, since it would make impossible the attainment or even the preservation of a high degree of organization. Without directive control, whatever is distinctively vital would soon disappear. It is a matter of general observation that in the absence of directive control all highly organized systems, non-living as well as living, tend to relapse into simpler or more loosely constituted systems, or as we say *dis*integrate. The question of the nature of such directive factors and their special distinction from random factors (such as those acting in Brownian movement) is fundamental for biology.

Let us begin by considering briefly a simple physical process, the process of *stirring*. In making a solution, it is usually not sufficient to place soluble substance and solvent in the same vessel; we must stir the mixture, i.e. subject it to continued agitation, the more random the better, for some time. Many experimenters in the course of chemical analysis have encountered fallacies resulting from incomplete stirring, but never from too complete stirring. What is chiefly peculiar about the process is its irreversibility. Once a certain stage is reached the mixture is homogeneous and remains so. Anyone who could then "unstir" it would be a miracle-worker; levitation would be a simple matter for him, since either feat would mean that the motions of the ultimate particles of matter, instead of remaining random, become definite and unified in direction, and that this definiteness of motion persists. Unification in the action of previously independent molecules would be implied; their individual autonomy would be lost, to be replaced by a togetherness, or integration, in which larger groups behave as single units. Segregation, differentiation, directive application of energy on a larger than molecular scale, and increase of structural complexity would all be possible consequences of such a substitution of definitely directed for random molecular motion.

In modern physical theory the irreversible feature of stirring is "explained" by considerations of probability. A simple model is the mixing of two equal sets of red and blue balls, similar in all respects but color and arranged in separate layers in a cylinder, by repeated inverting and shaking. The final arrangement, not easily altered by further shaking, is one where the distribution throughout the container is uniform; this distribution can be shown mathematically to be the most probable one. The rubbing out of a piece of writing by an eraser is a similar phenomenon. Reassembling the dispersed particles of graphite to restore the original writing would be an infinitely improbable event if left to chance. Eddington uses the more dramatic illustration of the typewriting monkeys restoring the books of the British Museum—if given enough time—by random hammering. These illustrations bring to our attention an important contrast between two types of arrangements met with in ordinary experience: (1) those formed by units acted upon by chance forces without unified control, and (2) those formed by the same units under some synthesizing or directive influence. An example is the contrast between a work of art and a smudge. There appears to be an automatic tendency in the natural world for highly individualized types of order to lapse into simpler or more generalized states—the process often called disorganization. The general physical condition formulated as the second law of thermodynamics is the same as that underlying the effect of stirring. Random motions of ultimate particles—in contrast to directed motions—lead inevitably to dissipation of energy and loss of ordered arrangement; the progressive natural trend toward uniform thermodynamic potential ("heat death") has its exemplification in the tendency of a solution to become homogeneous. Hence the restoration of any original ordered condition after dissipation or disorganization—e.g. unstirring a solution or unshuffling a pack of cards—is a most improbable eventuality, the term improbable implying that to do this within a reasonable time would require directive action,—i.e. a selection or a series of selections among many possibilities of action, all of which, in the absence of such direction are equally likely. Also—and this is

especially important—it would require the performance of external work upon the system, i.e., an addition to its store of free energy.

Let us now introduce some general biological considerations. The old rhyme of the sage of ancient Greece, (scratching out his eyes in a bramble bush and then unscratching them) has a quite special appositeness here and I quote it:

There was a sage of ancient Greece and he was wondrous wise.
He fell into a bramble bush and scratched out both his eyes.
But when he felt his eyes were out, at once with might and main
He leaped straight back into the bush and scratched them in again.

Here a complex organization, representing a highly improbable arrangement of many diverse components, is destroyed by the casual impacts of the thorns. This is possible enough. But to suppose that similar casual impacts would ever rearrange the dispersed elements of organization in the same order as before, thus restoring the sage's eyes, strikes us as the most complete impossibility. Nevertheless, according to our present physical conceptions, there is nothing "impossible" (in the strict sense) in the feat of the old man. The organization of his eyes is a consequence of the arrangement of certain components in a highly special kind of order. If we disperse these components and then reassemble them in the same order as before, the original system, with all its properties and activities intact, is restored. The theoretical assumption made by the old man—where he showed his wisdom—was that after any process has advanced to a certain stage the same process, if reversed in direction, will bring the system back to its original state. Hence, he reasoned, if you can scratch them *out*, you can scratch them *in*,—since "in" and "out" are relative terms relating merely to direction of motion. According to the rhyme, his wisdom was justified by the event.

Our present interest is in the problem of the conditions under which such a reversal could conceivably occur. If we examine into the presuppositions of any such process of reversal, we find that two are fundamental. The first is that the ultimate components of the system, the products of its finest physical sub-

division, are highly or even indefinitely permanent or stable. This stability applies to both their passive and their active characters. These components persist, with properties and modes of activity unchanged, through very great if not unlimited periods of time. The second presupposition is that it is always possible (at least theoretically) to place these stable components in any spatio-temporal relationships compatible with their physical characteristics, the only proviso being that each component has its own separate position in the system,—i.e., two or more cannot occupy the same space at the same time. Note that the word "always" means permanence or stability of condition; the statement just made asserts that those natural conditions which underlie the possibility of definite spatial and temporal relationships between physical units are permanent conditions. Essentially we have here an assertion of the non-temporal (non-changing) nature of the general or universal conditions (such as stability of class properties, additivity, separability, conjoinability) which are assumed as valid in logical or mathematical procedure,—an assumption always implicit in such procedure but usually taken for granted. In order, then, that a complex system shall have constant properties in its character as a whole, it must consist of stable entities in stable inter-relationships. Experiment shows that the exhibition of constant or characteristic properties by any composite natural entity (or system) does require both stability in the units composing it, and stability in the relationships between these components. Correspondingly we may distinguish two chief types of possible change or activity in any physical system: (1) when the spatio-temporal relations of the components change the properties of the whole system change, even though the components themselves (considered individually) show no change; and (2) when the special or intrinsic properties of the components change, the properties of the whole system again change even though (initially at least)[2] the relational structure remains unchanged. We have also the third case where change applies to both the intrinsicality of the components and their interrelations.

[2] Secondarily, of course, this relational structure may be altered if any component changes its properties or its activity.

We are here attributing primary importance to the persistence, stability, or durability of components and relations. A further supplementary consideration must not be overlooked, since in the natural world stability seems to be never absolute but always associated with some change. Nothing is more striking in our experience than the varying degrees of permanence in the different objects encountered in external nature or in the inner life. Some items have a massive persistence and unchangeability; e.g. stocks and stones, mountains, the heavenly bodies, or in sensory experience the special qualities of colors like blue; a similar permanence is shown by formal and mathematical characters like triangularity or the vexatious properties of the multiplication table. Other items, both physical and mental, are as evanescent as a puff of wind. And between the two extremes are all degrees. While some degree of persistence is necessary for any kind of existence—with (presumably) a minimal temporal qualification for each special kind of entity—we must note that an extreme if not indefinite persistence and stability are necessary characters of all entities or conditions which serve as elementary components or foundational units for syntheses of a complex or far-reaching type. The existence of unchanging foundational entities has long been recognized in philosophy (Plato's ideas, the various universals, Whitehead's eternal objects); and until recently science has regarded atoms as similarly unchangeable. The indispensable rôle played by conservation in physical nature is well illustrated, for the naturalist, by the Newtonian rule that every material entity has inertia. So long as an entity retains constant properties it retains constant mass; when isolated its mass and state of motion remain constant. In the presence of other entities the single entity may lose or gain energy; but the total quantity of energy in the larger system of which it forms part remains unchanged. The foundational status of energy in the physical world is recognized in the general scientific law which declares conservation to be its essential feature.

Observation also shows that while the individual entity can lose or gain energy, up to a certain point, without loss of its distinctive character, beyond this point its qualitative char-

acters change. This may follow because of increased interaction with other entities (i.e. those external to itself), or because of changes in its own internal condition; e.g. a metal melts above a certain temperature, or a photon changes its qualitative character (the associated color) when it gains or loses frequency. When an atom exchanges a quantum of energy with another atom (or receives or emits a quantum of radiation) the change in qualitative character in the atom may be slight—because of the enormous preponderance of intrinsic and non-interchangeable energy within the atom itself—but some change must be assumed to occur.

In the foregoing I have referred briefly to that feature of natural reality which appears to be the most general and fundamental, namely, the conjunction of permanency or conservation with change. We have seen that in any special instance the constant properties and ordered activity of the system depend on the permanence of its components and of their interrelations. But progressive change, as exhibited, e.g., in evolution, requires more particular consideration, especially in biology. Progress involves change, and progressive or evolutionary change is of a more radical kind than repetitive or routine activity. It is evident that any highly developed existing organization has been enabled to reach its present state by relying (so to speak) on the permanence of its components and on the permanence of certain definite relations which these components have established among themselves in the past; upon such secondary permanencies are superposed from time to time departures in various directions which may appear to be not previously determined. To account for these departures it seems necessary to assume the existence of spontaneous factors, "factors of novelty." Chief among these factors are to be considered those whose direction and time of application have a definite or unified as distinguished from a random character (directive factors).

In the case of the old man and bramble bush it is to be noted that the scratching-out process involves complete disorganization or destruction of some (typically the more complex) components, but not of others. Unless these persisted there would be no

possibility of restoration. We may imagine the sage jumping into a bonfire or furnace and staying there until completely incinerated—becoming the subject for an ash-analysis. His chemical as well as structural organization is completely destroyed; nothing remains but inorganic salts, CO_2, H_2O, NH_3, etc., and these are distributed at random. Yet we regard the stability of these molecules, and especially of their constituent atoms, as remaining undisturbed by the transformation; and the presumption is that if we were to reassemble the same atoms in the same relations as before we should restore the old man with all his faculties intact.

We must note, however, that nothing but a directive action of the most detailed kind could perform such a miracle. Such action, involving a selection among many possibilities of combination, is characteristic of synthesis, rather than of analysis or breakdown. In the dissipation nothing matters but the separation of previously interrelated components into any other kind of order (or "disorder"), and these kinds are infinite in number. But in the reorganization each one of these components (or its equivalent) must be brought back into a definite position in relation to all the others. In addition to being directive, the reorganizing activity must be completely unified or integrated. It is evident that any such integration presupposes directive action and bases itself necessarily upon such action.

Something of this kind is accomplished when from a nutrient solution, with components distributed randomly (and hence homogeneously), the living cell (e.g. a yeast or bacterium) builds up the special ordered arrangement—i.e. the chemical and protoplasmic structure, highly complex in kind—corresponding to its "inherited" organization.[3] We may imagine that we start with a suspension of living yeast cells or bacteria in water in a cotton-

[3] Just what "inheritance" implies need not be considered in detail here. We may simply note that the term recognizes the existence of some stable condition, transmitted through generations, which has a controlling and directive influence, constant and specific in kind, on the vital synthetic process. This condition ensures that all developing eggs of the same species pass through similar cycles of transformation under similar external conditions.

plugged tube, and that we heat this mixture to a lethal temperature, cool it again, and then let it stand. When equilibrium is reached, most of the original cellular components are dissolved and homogeneously distributed throughout the solution. This is the state which is reached, only more quickly, if we stir the mixture or subject it to any kind of random agitation. At this stage nothing further happens—at least nothing progressive; the system is stable. But is there any way of subjecting it to a directive agitation that will restore the dispersed components to their original relative positions and so recreate the living cells? This, in fact, is very easily done; all that is needed is to introduce one or a few living cells of the same species. At once a sorting or organizing process begins. From the disintegrated and dispersed components of the original protoplasm new cells are formed—as we observe the process, by budding or division from the introduced cells—and this process continues automatically until eventually the system regains something like its original state.

We witness (ocularly) a similar transformation of a random arrangement into an ordered arrangement when we observe an explosion in a reversed cinema film. We watch the crowd of casually distributed fragments—e.g. of a human being blown to pieces by a bomb-shell—following a course which finally places each component in its appropriate position as part of the complexly organized unity. The miracle of resurrection appears to be reënacted.[4] If it were possible to make our observation on a sufficiently magnified scale, especially with the use of the slow-motion apparatus, we should see each particle finally taking up a stable position in relation to the others by a most precise and definitely directed process of adjustment. At the last stage of all, where molecules are placed in definite relations to molecules in the fine-scale protoplasmic structure, factors peculiar to individual molecules or even atoms would be seen to come into play. Small-scale events would assume control of conditions; the field would become one of micro-physics rather than macro-physics.

[4] Cf. the description in the 37th chapter of Ezekiel.

We may take the reversed film as a graphic illustration or model of what occurs when the living organism builds itself up from the randomly distributed materials and energy present in the surroundings. Of course the appearance on the screen is an optical illusion of the most flagrant kind, and we know that all the stages of the sequence already exist in advance in finished form and orderly arrangement before the projection. The mechanism simply gives arbitrarily a sequence of visual impressions which is the precise reverse of those given by the natural sequence as originally recorded by the photographer.[5] Yet certain essential features common to all natural processes may be made clearer to our apprehension by considering just why the result strikes us as so completely impossible and paradoxical.

Consider again the cylinder with the red and blue balls. Starting with the balls sorted in two layers as before we make a moving picture of the whole mixing process, up to the stage of uniform distribution; then we reverse the film and study closely the movements of the balls in the image. We observe that although each ball shows many motions which have all the appearance of being random—occurring irregularly and impartially in all directions—there is also a tendency for all balls of the same color to shift their positions steadily in a constant direction, the red downward, the blue upward, until finally all are sorted in two separate layers and remain there. In this experiment we have, as it were, a model of a simple teleological process. Together with what may seem superficially to be an excess of purely random action, there is present a persistent directive and unifying influence making for the development of a definite kind of order (or organization) in the system.

We have the same general kind of appearance, only more striking, in the reversed film of the explosion. In both cases the apparent teleology is readily explained as a reversal of the normal temporal order in the succession of impressions. Ob-

[5] Incidentally we may note that any other desired sequence could be given to the separate pictures of the film. We have here an experimental confirmation of Hume's contention that there is no necessary connection in the succession of sense impressions through which we know external nature. But the distinction between real causal sequence and mere succession of impressions emerges all the more clearly.

viously in real nature the finished object exists in advance of its disintegration; when the order of events is reversed its image simply reappears at the end instead of the beginning of the sequence. Other appearances of the illusion are equally striking, especially the seemingly complete abrogation of the second law of thermodynamics. Materials unstir themselves automatically, uncool themselves, crowd closer together against resistance, assume the most improbable types of order. Yet all this occurs with consistency and regularity; it would be possible in principle to formulate a physics of the reversed film. This physics would be identical in certain respects with the physics describing real nature; for example its fundamental principles would include the persistence of matter and the conservation of energy; but the second law and all rules of natural action which are derived from this law would be replaced by rules of a totally different kind.

In both kinds of physics the possibility of orderly action and development would depend entirely on the presence and persistence of stable conditions. These would include (1) the geometrical properties of space-time, (2) the total energy content of the isolated system, (3) inertia and the other basic laws of pure mechanics, (4) the possibility of orderly interaction between separate entities (the general rule of causality) and (5) the retention of their special properties and qualities by the separate entities or systems in correspondence with their permanent features of constitution. I have already pointed out that in any act of construction or development the stability of certain entities and certain conditions is necessarily presupposed; unless these remain essentially unaltered throughout the period of assembling and arranging the components, all attempts at construction are futile. In so far, therefore, as any process of synthesis consists in the apposition or superposition of previously separated elements, the directive factors must have complete reliance (so to speak) on factors of stability of the kind just named. We conclude again that among the primary requirements for any orderly development or synthesis are (1) durability in the materials used in the construction and (2) constancy in the

conditions under which these materials are transformed and placed in position.

Just how are the general principles so far reviewed to be applied in the problem of vital action and development? The second law of thermodynamics would seem to be the expression of a character of natural existence which resists—or at least is indifferent toward—the production of diversity. Yet diversity appears in nature. The living part of nature shows a more minute and detailed diversification than the non-living part; and it is largely under the influence of living beings that the world becomes more and more diversified. The production of diversity, coëxisting with a trend toward integration or individuation, would seem to be a distinguishing pecularity of vital as distinguished from non-vital processes.[6] The question whether the second law is suspended locally in vital processes, as so many have supposed, is wrongly put, if not meaningless. The second law simply does not apply to local processes below a certain scale of magnitude and duration, since it is the statement of a mass effect, one in which the numbers of molecules are large, and random or non-unified molecular motions control the general flow of energy. Locally the motions (or other energetic manifestations) of single or a few molecules might have any characters required by the vital activity without infringing this law. It is quite reasonable to assume that the characteristically fine scale of protoplasmic activity and structure, with its minute diversification within a small volume, represents a device (so to speak) by which local processes are removed (at least temporarily) from the interfering influence of mass effects. The operation of factors whose cumulative effect is diversifying (of a sorting, "unstirring," or directive kind) seems clearly indicated by the observed facts of development; in any case the attempt to rationalize such facts as the automatic increase in complexity during ontogeny requires the use of other theoretical principles than those derived from the large-scale laws of energy.

[6] I am here making the usual distinction of natural science, and am not considering how far the natural property or activity which we point to when we use the term "vital" is present in latent form in inorganic nature.

Let us return once more to the red and blue balls. Suppose that, starting with the two separate layers as before, we found, after repeated inversions and shakings, that the two layers refused to mix and remained obstinately separate. What should we conclude? That the devil was in it? Experimentally, of course, we do not find such behavior in the model; but if it were to occur it would correspond, in a very definite sense, to the general biological fact of the retention of its characteristic organization by the living system in spite of external or other changes tending to disturb or disrupt that organization. So long as the system remains living, displaced or altered components tend to return automatically to their original state; the stability of the organization is thus ensured. The "living" state seems bound up with a regulative control having directive character. Some unifying factor is present which actively resists the tendency to disorganization. The special nature of this vital factor (or factors) must, needless to say, be sought out scientifically by the biologists, however much they may be reminded of the comparison between the "life principle" and Maxwell's sorting demon. What we note here is that the *maintenance* of a complex vital organization, equally with its attainment in development, appears to require a continual directive activity.

Development is the fundamental vital process. Any special biological organization, with everything that it implies and carries with it, is a temporary construction, built up progressively from apparently simple beginnings in which little or no corresponding organization can be discerned; this organization is maintained (as we have just seen) by unremitting vital activity. Ontogeny, like any other constructive process requires as its preconditions (1) stable properties in the materials with which it works and (2) stability in the conditions under which these materials are transformed and disposed. Together (1) and (2) may be classed as factors of stability.[7] But evidently stability by itself can effect no change, and other factors must be held responsible for the actual work of transformation. These factors

[7] *Cf.* my paper, "The Problem of Vital Organization," *Philosophy of Science*, 1934, Vol. 1, p. 296.

we characterize in general terms as active factors or factors of change, which operate under restrictions imposed by the stable factors. In the particular case of the vital process an additional requirement is that the incidence or detailed application of the active factors should be not random or uncoördinated but definitely directed and unified. Apparently the place, time and direction of application are determined, as regards the single components and events (or event-components[8]) of the system, by some more inclusive stable condition which exists in advance of the process and exercises some kind of comprehensive unifying control over its whole course. At the present time most biologists identify this controlling condition with the special influence exerted by the nuclear gene complex, which is regarded as existing in its entirety in the chromosomes of the fertilized egg-cell and all its descendants (see below).

We are familiar with a controlling condition of a broadly analogous kind in the general plan or design governing any consciously purposive human undertaking; this constitutes a pre-existent stable factor which coördinates and integrates the separate events of the whole constructive sequence. When the builder is uncertain what to do he refers to his plan; under similar conditions the scientific experimenter refers to his working hypothesis. In either case the reference is to something fixed (at least relatively) and present in advance. If the scientific experiment confirms the hypothesis the existence of some stable controlling condition in an independent nature is indicated; and the hypothesis may then be reconstructed into a theory (as often happens), this being a descriptive statement or formula of some fundamental natural permanency. The working toward the realization[9] of some final "end," or station of at least relative stability, which in some sense exists in advance as a controlling and unifying plan, pattern or principle, is the general characteristic of a teleological process. We have to inquire in what sense the living organism is an instance of such teleology.

[8] Some such term seems needed to denote the single active events which combine to form larger processes.

[9] *Existentialization* might be a better term than realization, although it is not in the dictionary.

Why should the behavior of the particles in the reversed film (of the explosion) seem to the naturalist so completely impossible and paradoxical? At least two fundamental reasons can be seen. First we are disconcerted when we see a crowd of disconnected particles converging toward one another, apparently spontaneously, to occupy smaller space. We know from the general behavior of compressed gases (the driving force of most explosions) that minute scattered particles, especially molecules, do not converge automatically except under the influence of some outside force; they tend rather to diverge.[10] All materials fly apart at high enough temperatures; cohesion enters only when the molecules are close together and the temperature is low. To bring dispersed molecules closer together requires work of compression: we feel this intuitively and cannot see where this work is to come from.

But why is work needed to compress, while expansion is automatic? Such a question, if pursued far, leads to highly fundamental considerations, although the immediate answer seems simple enough. Evidently because each particle occupies space; and since it conserves its existence, i.e. has a certain constancy of mass and properties associated with its durability, no two particles can "occupy" (as we say) the same space at the same time. All physical existence means occupancy of space and time, together with individual character or uniqueness (individuality) on the part of the occupant; accordingly interference is a fundamental fact in nature. We picture the particles as flying off from one another when they strike; in a crowd of particles in random motion these mutual impacts force them from positions where encounters are more frequent to those where they are less frequent; i.e., according to the kinetic theory of gases, from regions of higher to regions of lower pressure. To reverse this tendency work must be applied from without. It is important to note that the ultimate reason for this condition is the possession of energy in a highly concentrated form in each individual particle. Mass is identified with energy in modern

[10] Gravitational convergence may be neglected in this case in comparison with the kinetic energy leading to expansion.

physics. As to the source of the intra-atomic energy nothing scientific can be said at present; we must accept atomism as an ultimate fact of nature. At least it indicates that certain definite conditions making for convergence and localization of energy, in addition to conditions which favor dissipation and disintegration, are fundamental in natural existence. Conditions of the former kind may be regarded as of the same ultimate nature as those underlying other kinds of directive action, which similarly involve convergence and localization.

The second surprise in the reversed film is to see the components of the eventual system passing spontaneously from a state of incoherence and disorder to one of coherence and definite order, i.e., organization. In our human experience order, coherence and unity cannot be imparted to a random collection of materials without the exercise[11] of conscious design and selective action. Teleology without a directive and integrative agent is hardly conceivable. And since no such agent is visible in the film picture, the latter seems impossible as a portrayal of actual nature.

The whole phenomenon would seem unaccountable if we did not know that a directive and unifying condition exists in advance. In reality the whole sequence is already laid down; its successive stages merely become visible in order as the mechanism unfolds the film. In this instance the apparently teleological character of the synthesis is satisfactorily explained, and it may be noted that the explanation is one of preformation. This raises the question: is it necessary to assume preformation of some kind as the controlling factor in ontogeny? In so far as the adult organization is referred back to the genic organization this would seem to be the case; but we defer consideration of this problem for the present.

Let us return to the synthetic action of the living organism in the culture medium. Here we do not have any scientific difficulty in accounting for the work of compression. Such work is

[11] That is, at some time or another. Sorting and ordering operations may become automatic and unconscious through habit; i.e., the conditions which render such operations possible become stable through repetition; or sorting machines may be invented.

continually being performed in concentrating within the growing cells the diffuse materials of the surroundings. Additional work is required for certain chemical syntheses. But we have in the oxidative metabolism of the cell a source of energy demonstrably sufficient for all these activities. While it lives the cell oxidizes food materials and liberates energy. All biologists recognize that a part of this free energy is applied in the upbuilding and maintenance of the protoplasmic system. Work is involved in the collection of food materials from the surroundings, in the details of their chemical transformation, and in the disposition of the structure-forming and other reaction-products in their proper places within the cellular organization. In association with this active collection and incorporation of materials essential to the protoplasmic make-up there seems also to be an active prevention of their loss to the surroundings. Such loss is also passively retarded or prevented by the presence of diffusion-proof barriers between the cell and surroundings, such as the plasma-membrane.

But in the work of the maintenance and especially in the upbuilding of the cellular organization other factors enter, and many of these are clearly of the directive kind, especially those responsible for the finer biological organization of the system. How can these be conceived in the light of our present knowledge? The first step in answering such a question is to inquire what general evidence we have of the presence of directive or formative factors in the elementary physical processes of nature. Apart from the basic fact of atomism, to which I have already alluded, we find ample evidence of this kind in the behavior and structural characters of many forms of matter. It is easy to demonstrate experimentally the existence of constant factors whose action is to collect randomly distributed materials and place them in definite structural associations and groupings. This is seen in the universal tendency of solid materials to adopt a crystalline structure, e.g. when deposited from solution; crystals, according to modern physics, represent the form assumed spontaneously by close-packed aggregates of similar uniformly oriented molecules. Crystalline structure is thus an index of molecular

structure; this, in turn, is dependent on the structure and mutual affinities of atoms; atomic structure is again to be referred to the properties and grouping of the intra-atomic electric units. Further regression—i.e. further extension of subdivisional or whole-part analysis—is theoretically possible but without experimental basis at present.

There has long been a tendency among biologists to regard crystallogenesis as an essential factor in biological morphogenesis. In other words, those conditions which determine the aggregation of molecules to form specific crystalline patterns have been regarded as ultimately responsible for the specific morphological patterns built up by living organisms through their protoplasmic activity. According to such a view, biological morphogenesis and crystallogenesis are at bottom the same; or at least the former requires the latter as its indispensable precondition. Briefly put, the conception is that the structural materials of organisms, when laid down in or between cells in solid form, dispose themselves automatically in definite patterns which themselves are determined by the molecular patterns of the constituent compounds. These crystalline patterns determine the morphological characters of the larger solid aggregates, whether these form solid crystals, liquid crystals, or the nematic forms characteristic of many chain compounds. Specific chemical character implies specific structural character; hence this view gives a reasonable explanation of the observed correlation between biological specificity and the chemical specificity of proteins. A growing organism assumes a constant form just as a growing crystalline complex assumes a constant form, and for the same ultimate reason. To most biologists, however, this conception of morphogenesis appears insufficient, for although we may agree that the finer elements of organic structure are themselves structural in the crystallographic sense, such considerations throw little light on the problem of accounting for the special morphology exhibited by the organism as a whole—e.g. a man as distinguished from a tree.

Nevertheless it is a fact of fundamental significance that the tendency to adopt definite and stable form is shown by natural

systems of the most diverse kind, from the smallest to the largest scale. We meet here with the problem of the nature and origin of those general conditions which underlie the tendency of natural units or aggregates to assume characteristic forms. Morphology, in this general sense, appears fundamental in nature; i.e., structure, the existence of stable interrelations between parts, is a feature of every enduring physical existent. Such interrelations presuppose stability in the parts themselves, as well as in their interconnections. Larger parts appear always to be composed of smaller parts, and these of still smaller, as far as spatial analysis extends (Principle of Concretion or Concrescence).

But we may ask the question: when we carry subdivisional analysis as far back as possible, what kind of stability do we find in the elements finally reached? To physics these appear as centres of vibratory (regularly recurrent) activity in which all that appears stable is the *pattern* of the activity, i.e. its periodicity and wave-form. Stability of formal conditions is combined with incessant activity in the actual entities themselves. Elementary or atomic existence appears as activity, but as activity confined within rigid formal conditions.[12] The stability of these conditions seems very great, if not absolute; they appear to be an integral and invariant part of natural existence. The whole of physical evolution has as its indispensable condition or background certain enduring natural characteristics, conspicuous among which is the tendency toward form and the persistence, i.e. stability and indefinite reproducibility, of certain forms, once they are attained.

What is implied by the stability of formal conditions? Just what experientially is "form?" and how have we reached the concept of form as something different from—and perhaps having a reality independent of—concrete actuality? At least it seems clear that the concept of form and the concept of stability are

[12] The kind of rigidity or constancy which we ascribe to these purely formal conditions is different from that ascribed to physical fact. It is more of the nature of permanent possibility, and requires to be supplemented by something else, i.e. activity, in order to yield the physically real or existent. It is to be noted that permanent possibility requires permanence in the character of the ultimate or foundational reality.

closely allied. No form would be possible in Chaos. A formal pattern represents a permanently possible arrangement of parts. Historically we derive the science of spatial form, geometry, from the experienced properties of solid bodies. Solidity and stability tend to be identified in our minds. Things that persist, that resist deformation, and can be used again and again (like the implements of daily life) are things with definite form. Hence form, *per se*, has come to be associated with stability, and finally is regarded as having a stability on its own account, even though a given material object (e.g. a puff of smoke) may exemplify a given form only transiently. Thus the possibility of entering into definite formal patterns and retaining these for some time appears as a general property of matter, inherent in its spatiality. A quantitative stability or conservation, surviving all transformation, has long been regarded as applying to matter in the same sense as to energy. This is not surprising if we identify these two types of physical existence. To suppose, with Plato, that forms exist (except in the sense of potentiality) independently of real objects may be a fallacy of abstraction. Nevertheless it is a real cause for wonder that the forms assumed spontaneously by so many natural objects, especially pure materials in the solid state, should be so simple, regular and constant. Since definite structures—like the cubes of sodium chloride—appear constantly whenever pure substances solidify, and since such repetition implies an underlying stability of condition, we seem obliged to recognize as existing in nature a permanent or inherent background of general or all-pervading stability,—i.e. some kind of uniform matrix which forms the condition for the continual reappearance (occurring independently of special place and time) of the same regular structures. Simplicity and symmetry seem to have a preference over complexity in the elementary forms of nature, as is shown for example in the spherical form of objects so different as fluid droplets and the heavenly bodies. The directive action which arranges Na and Cl atoms in a cubical lattice has an analogous symmetrical character; and presumably snowflakes have the same shape in the moon or the companion of Sirius as on the earth. Such con-

siderations furnish good ground for assuming homogeneity or isotropic properties in the general matrix or continuum of physical nature;[13] and, as we have seen, the facts of crystal structure imply simplicity and symmetry not only in the structure of molecules but also in the internal structure of the atoms themselves, or at least, in the forces by which each atom acts externally to itself.[14]

Summarizing, we distinguish in inorganic morphogenesis two broad groups of factors: (1) the internal structural and other characters of the molecules themselves, and (2) the external conditions under which they cohere to form larger aggregates. The general tendency to formal simplicity is referred to the isotropic character of the general space-time continuum, on the one hand, and to the internal regularity of molecular structure and activity on the other. Departures from symmetry in crystals are referred (assuming uniform external conditions) to asymmetric internal conditions in the molecules: as an example we have the well known difference between laevo- and dextro-crystals. The special shape adopted by the aggregate is thus dependent on the internal or intrinsic characters of the molecules, these characters being in turn dependent on the intrinsic characters of the atoms, now conceived as systems of electric units. The characters of atoms determine their modes of interaction and hence the structural and other characters of the combinations which they form. Subdivisional analysis cannot be carried beyond this limit at present.

In biological morphogenesis directive activity is seen in its most highly developed form. It is said that physics does not concern itself with intrinsicality,[15] but considers natural fact in

[13] The conception of a homogeneous continuum in which natural objects are set and through which they interact corresponds to the older physical conception of an ether.

[14] It may be noted that simplicity and symmetry in the external manifestations of a physical unit are not necessarily a proof of internal simplicity. In its intrinsic character even an atom may be indefinitely complex, but this complexity does not enter directly in determining the type of arrangement formed by groups of atoms, provided the ground plan is simple. Similarly military formations may have a simple lattice-like character, in conformity with the flatness of the parade ground and the uniform shapes and sizes of the soldiers, each of whom is nevertheless a complex individuality.

[15] *Cf.* Bertrand Russell: Outline of Philosophy, eg. pp. 108, 148.

its external or public aspects only. Life, however, remains essentially intrinsicality;[16] any comprehensive theoretical biology must base itself equally on its internal (e.g. psychological) and its external aspects. How is this special intrinsicality of the living organism to be related to its physical characteristics,—i.e. to the side with which physiology is concerned? As regards morphogenesis a partial answer is indicated above: since the essential directive forces in crystal-formation are forces internal to the molecule, and since underlying the universal vital property of morphogenesis is the universal tendency of solid compounds to be deposited in definite structural form, a reference to the structure-determining factors intrinsic to the biochemical molecules is implied. This answer may be true so far as it goes; there can be no doubt that biological specificity and biochemical specificity are closely inter-related; but no modern biologist would regard such a reference as sufficient. It is clear that the precision of the morphogenetic process in ontogeny implies a corresponding stability in the controlling factors, but at present the prevailing theories of genetics refer the intimate control of this process and the final attainment of definite specific form to stable factors of a different and more complex kind. These factors are the genes—stable units, limited in number, whose existence seems clearly established by the facts of Mendelian inheritance.[17] Their special individual properties, together with the combinations which they form, are regarded as determining the detailed nature of the morphological and other characters which emerge as the developing organism grows from germ to adult. These stable controlling or directive units are nuclear, i.e. have a central position in the cell; in some way they act as factors of stability which confer unity and regularity on the complex sequence of coördinated biochemical and morphogenetic processes underlying development. The physiological problem of just how this control is exercised is quite unsolved; although it is evident that any such control must be through an influence on

[16] Compare A. N. Whitehead: "Nature and Life." University of Chicago Press, 1934.

[17] *Cf.* the recent address of H. S. Jennings, "*Fundamental Units in Biology*," Science, Vol. 84, 1936, p. 445, for a discussion of the general characteristics of genes.

metabolism. We cannot, however, dissociate the "formative" and the energy-yielding metabolism in any ontogeny. The presumption is that given a certain stable combination or routine of biochemical reactions, and certain constant structural and other conditions under which these reactions proceed, the solid reaction-products are laid down in definite sequence and definite structural inter-relationships, and a definite type of morphology is the result. The dependence of the finer morphological detail on the molecular configuration of the structure-forming compounds is always presupposed; the observed relation between the chemical specificity of proteins and the species specificity is an expression of this interdependence. But superposed on this purely physical condition there appear to be controlling and unifying factors of a more comprehensive and more active kind which exert their influence in the manner we call directive. Since these factors pervade the whole organism, at all stages, the only physical substrata to which we can point would seem to be the genes, which according to the existing evidence are present in full quota in every cell nucleus.

Gross morphology is regarded in modern biological science as an effect of the summation and coördination of the separate cellular processes, which act in an integrated manner under the control of special physiological conditions having a large spatial and temporal field of influence. In modern genetic theory, as we have just seen, these conditions are referred ultimately to the genes, but we can say little at present regarding the nature of gene action. Since the genes are nuclear in their situation, while the structural and physiological characters of the cell are chiefly cytoplasmic, it is assumed that a controlling nuclear metabolism must lie behind the cytoplasmic metabolism; i.e., there is interplay between the metabolic processes in cytoplasm and nucleus, but of these two reaction-fields the latter holds the master position. The ultimate directive action is thus referred to the nuclear units or genes. These are highly stable in their chemical constitution and behavior, and the dependent cellular processes, including the morphogenetic sequences of development, exhibit a corresponding stability. The power of indef-

inite reduplication, while retaining chemical and other characters intact, is essential. So long as the genes multiply and preserve constant properties the dependent characters of the species remain also constant.

I do not propose to offer a new physiological theory of the action of genes,—i.e. a theory of the physico-chemical conditions under which they exercise their control. This controlling action has been compared with catalysis (especially autocatalysis) and it can hardly be doubted that catalysis, as a general factor in all chemical reactions, is an indispensable precondition of gene action. I.e., gene action would not be possible without catalytic action; yet it is apparent that gene action has its own special characteristics which are not to be identified with catalysis but rather are superposed upon the latter and presuppose it—very much as vital action presupposes chemical action. Such a conception would regard the genes as themselves living or metabolically active units, the seats of certain highly stable master reactions which have a determinative relation to the whole biochemical sequence underlying development and maintenance. Muller[18] and others have recently regarded the genes as autonomous living units and have compared them with ultravirus particles, but this comparison simply emphasizes the autonomous and specifically vital characteristics of genes, without contributing further to the solution of the problem. We may here note that when we refer the ultimate control of vital characters to genes, which physically are ultramicroscopic units, any attempt at further physiological analysis means entering the domain of small-scale physics. Here all methods of observation become more and more subject to error and disturbance as the spatial dimensions decrease. The final origins of the impulses to action are beyond observation; and the directions in which they are applied in single cases cannot be externally controlled. We must assume that constant factors play a part in these actions, since the outcome is constant,—i.e. that they occur under conditions which are stable and which limit definitely the possible

[18] *Cf.* H. J. Muller: 'The Gene as the Basis of Life,' Proceedings of the International Congress of Plant Sciences, 1929, Vol. 1, p. 897.

range of activity. These controlling conditions must persist, otherwise, reduplication would not be possible, but we have at present no scientific knowledge regarding their nature.

Finally we have to recognize also the existence of spontaneous or unforeseen actions in nature, furnishing the conditions for diversification and creative evolution. These actions evidently have directive character, and they seem to reach a special focus and intensification in living organisms; but for their ultimate origination we can only point to the metaphysical world, as Claude Bernard said many years ago. Any possible metaphysics of living nature would require a quite different kind of treatment from the foregoing and would lead to considerations which would be beyond the scope of the present article.

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Psychology as a Science of Objective Relations[1]

BY

EGON BRUNSWIK

I

THERE is sufficient evidence from more recent experiments in psychology that equal retinal stimulus-elements do not lead to equal experiences and reactions except under certain rather specific conditions. An unsophisticated observer will find himself surprised to be able to cover with his own finger a person entering the door of his living room. When the finger is moved to the right or left, thus doing away with the precise retinal coincidence with the person, the observer will soon become unable to recognize intuitively the actual retinal stimulus equality of the two distant things. This usually holds even for the case when he is making every inner effort towards an antagonistic, analytic perceptual attitude of the type which is used by painters or draughtsmen in order to represent the environmental situation in a similar way as it would project itself on a photographic plate or on a retina.

[1] This article was read, with minor changes, before the Cosmos Club of the University of California, at Berkeley, April 1936.

It is a short outline of some of the more general considerations made in the author's "*Wahrnehmung und Gegenstandswelt—Grundlegung einer Psychologie vom Gegenstand her*" (Leipzig, Deuticke, 1934).

A more extended presentation of the connected experimental research will be found in a series of studies edited by the present writer under the general title "*Untersuchungen über Wahrnehmungsgegenstände.*" Until now the following titles have appeared:

Instead of this, the unconstrained observer will find it easy and natural to perceive and to compare bodies satisfactorily with respect to their own measurable physical sizes, regardless of all changes in distance or spatial orientation. For a somewhat developed human being, an approaching visitor will not grow from a tiny fingerlike dwarf up to an immense giant, but will, within certain limits, quite fairly retain a constant apparent size. This "body-size constancy" (despite changes of distance and therefore of retinal stimulus-sizes) is under normal conditions a fairly reliable, deep-rooted, well-established, broadly supported habit of the perceptual system. It also holds almost equally well for objects not as familiar in their bodily characteristics and even under conditions of reduced clearness in spatial organization.

Perceptual constancy is not limited to the type of physical property ("object") called "body-size." In connection with changes in special orientation, even the shape of a body will be radically distorted in its retinal representation; for instance, a circle in oblique orientation will project itself as an ellipse. Despite this it usually will still appear in the "Gestalt" of a circle and the projective distortion will ordinarily not even be noticed. Furthermore, a piece of chalk placed in shadow may send equal

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I. E. Brunswik, Die Zugänglichkeit von Gegenständen für die Wahrnehmung und ihre quantitative Bestimmung, *Archiv für die ges. Psychol.*, 1933, **88**, 377–418.

II. B. E. Holaday, Die Grössenkonstanz der Sehdinge bei Variation der inneren und äusseren Wahrnehmungsbedingungen, *ibid.*, 419–486.

III. K. Eissler, Die Gestaltkonstanz der Sehdinge, *ibid.*, 487–550.

IV. S. Klimpfinger, Ueber den Einfluss von intentionaler Einstellung und Uebung auf die Gestaltkonstanz, *ibid.*, 551–598.

V. S. Klimpfinger, Die Entwicklung der Gestaltkonstanz vom Kind zum Erwachsenen, *ibid.*, 599–628.

VI. T. Izzet, Gewicht und Dichte als Gegenstaende der Wahrnehmung, *Archiv f. d. ges. Psychol.*, 1934, **91**, 305–318.

VII. K. v. Fieandt, Dressurversuche an der Farbenwahrnehmung, *Archiv f. d. ges. Psychol.*, 1936, **97**, 1–30.

In English, a very brief sketch emphasizing the empirical aspects has been given under the title "Psychology in Terms of Objects" (*Proceedings, Anniversary, University of Southern California*, Los Angeles, 1936, 122–126). Furthermore, some points are brought out in a joint article of E. C. Tolman and the present writer, "The organism and the causal texture of the environment," *Psychol. Rev.*, 1935, **42**, 43–77.

or even less intense light-stimulation into the eye than does a piece of coal which lies beside it in direct sunshine. And yet under a normal, clear survey of the situation, the chalk will appear white and the coal black, corresponding to their invariant physical color-properties of high vs. low reflectivity. Quite similar effects to size-, Gestalt- and color-constancy have been found to exist in the field of audition, where the apparent loudness of a sound-source remains approximately constant even if the distance —and therefore also the intensity of the sound-wave which arrives at the ear-drum—is subject to considerable changes, etc.

In each of these cases the perceptual system proved itself able to establish, in a fairly approximate way, a constant, i.e. a one-to-one coupling between physical characteristics of distant bodies, on the one hand, and its own perceptual or motor reactions, on the other.[2] At the same time it succeeded in making itself independent from the varying relations of these distant bodies to the observer (e.g. distance) or to other objects (e.g. the sources of illumination). This effect is of extreme biological importance to the organism, since otherwise no orderly and self-consistent "world" of remote manipulable "independents"[3] could be established; the physical and topographical constants of the environment would be completely lost in the random variations of their

[2] It is essential to connect, in any case, the use of the term "constancy," as it became customary in psychology, with a further clear conceptual determination of the type of abstract (measured or computed) type of physical property or "object" (*Gegenstand*) for which the "constant" coupling to a certain type of reaction is successfully established.

Gestalt psychology, in its successful fight against the "*constancy-hypothesis*" (which has been an unrecognized premise of the old associationism), emphasized that there is no one-to-one correlation between retinal ("proximal") stimulation-elements and perceptual reactions. The same negative statement of an absence of a "retinal-size constancy" was the topic of our first introductory paragraph. This is far from being in contradiction to the positive statement of a body-size constancy. In fact, abandonment of the "constancy-hypothesis" even cleans away for finding that other physical something—body-size—which does stay in a (fairly good) one-to-one relationship to the reaction and thus easily presenting the environmental terms which gives proper sense and meaning to the establishment of the reaction.

[3] The term "independent" was used by E. C. Tolman ("Psychology versus Immediate Experience," *Philos. of Science*, 1935, 2, 356–380) in order to indicate types of objects whose definition does not include a reference to a relationship of an environmental entity to the organism.

"proximal" stimulus-representations.[4] No "things" (as, e.g., comrades, the enemy, food, tools) could be recognized as identical, when looked at under changed circumstances, since all their characteristics may have changed their actual projective values.

From the standpoint of an observing physicist or behaviorist, the stimulus-projections of the bodies on the retina belong to the effect-pattern which is sent from all parts of the total environmental situation to the stimulus-surface of the observing organism. Since this total effect-pattern is the only source for a correct orientation and reaction of that organism, the couplings which could be headed under the concept of "thing-constancy" must be mediated by it. In searching for characteristics of the total stimulus pattern which would be able to represent bodily characteristics in a fairly unambiguous way (e.g. body-sizes regardless of distance) we find certain sets of combinations of projective sizes, on the one hand, with distance cues (such as binocular disparity, perspective distortion of right angles, etc.), on the other. A body of a distinct size may be represented in different ways: either by a certain small retinal projection connected with a stimulus-cue for a certain large distance or by a certain large projection connected with a cue for a certain smaller distance. The same holds for the representation of the physical color of a body by combinations of the varying intensities of its projective retinal stimulus-value with cues for the corresponding actual conditions of illumination. The need for such a twofold (at least) stimulus-basis for the causal mediation of every kind of thing-constancy was pointed out by Bühler in his "duplicity-principle."[5]

An organism which has established a system of reactions which appear to depend in a fairly constant manner on body-character-

[4] The term "proximal stimulus" was used by K. Koffka ("Principles of Gestalt Psychology," New York, Harcourt Brace, 1935), in order to discriminate the stimulating event arriving at the sense organ from the "distant" body. For the sake of brevity we use the term "stimulus" always in the sense of proximal stimulus or even of primary physiological excitation, whereas the remote manipulable cause of the stimulation will be called "body" or "body property." Both, stimulus as well as body property, are types of physical entities or "objects."

[5] *Cf.* K. Bühler, *Die Erscheinungsweisen der Farben*, Jena, Fischer, 1922, and E. Brunswik und L. Kardos, Das Duplizitätsprinzip in der Theorie der Farbenwahrnehmung, *Zeitschr. F. Psychol.*, 1929, 111, 307–320.

istics as such, regardless of their actual retinal projections, has proved therefore two abilities: (1) to make use of indirect stimulus effects of bodies as cues or signs indicating their presence, (2) to integrate distinct stimulus-elements into unified functional wholes which act as a single unitary basis for further reactional effects. Both statements follow from a purely objective, physicalistic analysis of the type of achievement or success involved in the facts of thing-constancy. Since the first statement refers to the notion of meaningful representation by signs and the second to the totalitarian *Gestalt* principle, this way of functioning is best comprehended in the proposition of Tolman, that the intercourse of the organism with its environment takes place in the formation of "sign-Gestalten."[6]

The way in which this effect is attained may be compared with the functioning of a collecting lens. The single central ray arriving at a particular point does not allow any inference as to the distance of the point wherefrom it is starting. A possibility for such an inference will not become granted unless other partial-effects of the situation—that is, for our present case, the marginal rays—will be brought, by the collecting lens, from their original divergent status into convergence and finally to intersection. From a knowledge of the point of intersection and the distance, curvature and material of the lens, a fairly unambiguous statement regarding the location of the radiating point can be obtained.

An analogous general way of functioning is true for the case of perceptual thing-constancy. As has been pointed out, the gross characteristics of the direct retinal projections of the physical bodies in question are, *per se*, unable to represent in a satisfactorily unambiguous manner their sizes, forms, colors (reflectivities) etc. Certain particular further traits of the effect-pattern which reaches the retina, indicating distance, illumination, etc., have to be included in order to accomplish this task. These "cues for the circumstances" (*Umstandskriterien*) play, therefore, the rôle of the marginal rays in the case of the lens. The lens itself is represented in our more generalized case by the eye together with the optical sector of the nervous system. Corre-

[6] *Cf.* E. C. Tolman, *Purposive Behavior in Animals and Men*, New York, Century, 1932.

sponding with the more complex nature of the task, even the integrative action has to be spread out into more complex and extended patterns. The lens of the eye, which is involved in this system of procedures, is but a part of this bringing together of differently spread-out and randomly diverging effects of the situation which is to be mastered in regard to its behaviorally important physical traits.

The couplings established in thing-constancy, therefore, appear to be a particular complex case of a causal relationship. The characteristics of the bodies in the distant environment, on the one hand, and the final (perceptual or motor) reactions, on the other, are connected with each other by a texture of causal chains first diverging and then brought to a new convergence and to intersection at the point of reaction. Such a mechanism of "*multiple mediation*" may grant a one-to-one correspondence between object and reaction for all the variety of circumstances under which the object can be perceived.

The most important trait of this one-to-one causal relationship seems to be that, at certain mediating cross-sections of the causal texture mentioned, no single event will participate in it. Size-constancy, for instance, means that, e.g., an 8-cm. body will, under a great variety of conditions, always (approximately) be responded to as an 8-cm. body, that is, be recognized as such. There is a certain set of correct "8-cm. responses" given in all cases where an 8-cm. body is present. But nevertheless the direct retinal stimulus, which is an element of the mediating causal texture, will vary within a wide range corresponding to the actual distance of the body.

Along with the variability of the retinal projection and indirectly proportional to it, the corresponding stimulus-cues indicating the distance of the body will also vary.

Besides this variation which compensates for the variation of the direct retinal image another kind of variability of the distance-cues will be noticed, which may be even more interesting from the standpoint of the sign rôle which the proximal stimuli have to play in mediating the environment to the organism. One and the same distance may be represented once by a certain amount

of binocular disparity, another time (or even simultaneously) by a certain amount of perspective distortion of bodies of right-angular form, etc. Organisms have learned to use a large variety of cues in a *vicarious* manner, especially where a certain life-important type of fact is functionally difficult to attain, i.e., where simple, always present, and unambiguous cues are not available. The cues thus belonging to one "*cue-family*" may have nothing in common among themselves, *per se*, i.e., in their intrinsic, geometrical properties. They may be connected by nothing except their indicating value for a more or less probable common cause which is remote from the stimulus-surface of the organism.[7] The formation of such an "or-collection" (*Oder-Verbindung*) of coordinate cues (or cue-configurations) cooperating and substituting for each other in releasing identical behavioral consequences without being similar among themselves, seems to me to constitute an objective *operational criterion for the fact that these stimuli have received "meaning"* for the organism by being admitted as signs for something else.[8]

To characterize these important features of multiple and variable causal mediation, we may call the constant far-reaching couplings between objects and reactions "interruptable." There are two focal (or modal) regions, or kinds of events—objects and reactions—, and the spatial and temporary gap between them

[7] According to their objective probability cues may be graded on a scale of "reliability." No cue, of course, is perfectly reliable, i.e. inimitable and therefore univocal in its indicative value. One may think of a stereoscope counterfeiting a cue of such high reliability, as binocular disparity is.

[8] It has been shown by Holaday (*l.c.*) that binocular disparity is able by itself to sustain a high degree of size-constancy, whereas its elimination (by closing one eye) becomes almost ineffective in cases in which the full normal variety of other possible distance-cues is available. A similar and very perfect "substitutability" of tactual and visual cues for the volume of a body has been found by Izzet (*l.c.*) in experiments on weight-constancy.

The cognitive concept of the "or-collection" building up a "cue-family" has its parallel, on the action-side, in the concept of the "habit-family-hierarchy" of C. L. Hull ("The concept of the habit-family hierarchy and maze learning," *Psychol. Rev.*, 1934, **41**, 33–54). The term "hierarchy" is—in both cases—apt to indicate the differences in "goodness" or "reliability" of cues or means respectively. W. S. Hunter ("The psychological study of behavior," *Psychol. Rev.*, 1932, **39**, 1–24) made the fact of "vicarious functioning" a central point in the distinction between psychology and physiology.

appears to be in some sense over-bridged by their constant coupling, since at the mediating layer of retinal stimulation (and of primary physiological excitation) none of the types of single cues do correspond with either of these classes of events. But nevertheless the whole process can be completely understood in terms of a certain type of physical process, as symbolized by the case of the collecting lens.[9]

There is, of course, one common abstract feature in the total stimulus configuration which is actually mediating body-size to the organism: a certain mathematical function (product) of the visual angle, as indicated by the retinal size, on the one hand, and the distance, as indicated by the particular nature of the distance-cues available, on the other. If we should not be able to find such a common trait, which would be—within the limits of the reliability of the cue-elements in question—in a one-to-one relationship to the attained type of object, "body-size," the whole achievement would remain miraculous. There is no discontinuity in this sense. But, taken as concrete events, there is a large variety of functionally discriminable mediating cue-configurations, as compared with the undifferentiable equality within the field of the releasing remote object-property, on the one hand, and within the behavioral output, on the other. In this sense, the strain of univocality between object and reaction is indeed interrupted.[10]

[9] The only question remaining open for a physical explanation is as to how natural or artificial tools (or organismic "institutions") like collecting lenses, or the even more complex organismic systems functioning in a similar integrative way, might have developed at all. This general genetic question of living organization belongs to the field of theoretical biology and the psychologist does not need to be concerned with it, since his problems are centered more around actual achievement and functioning.

[10] Multiple mediation—i.e. checking as much as possible all variations in the situational circumstances—is one way of rendering "far-reaching" couplings undisturbed by interfering conditions of causation. Another practice would be that of keeping all conditions of observation actually as constant and insulated as possible and let no uncontrolled "lateral" causal chains interfere. This latter would be the procedure usually followed by physical as well as by the traditional psychophysical experimentation, and also by man-made machinery, as e.g. electrical transmission of a message. Here the univocality remains obvious along the whole chain of mediation. This second procedure is in general the more reliable one, but also the more round-about way as far as the single case is concerned.

Psychology deals with the abilities of organisms to establish intercourse in a successful way with the surrounding world, in reception (cognition) as well as in action. It seems, now, that a most essential description of (receptive) abilities could be given by differentiating as clearly as possible the types of physical properties ("objects") to which the organism is able to react in a fairly undisturbable, univocal manner, or which, in short, he is able to "*attain.*" Let us return again to our example of size-perception. There the alternative would be whether, as a decisive type of object influences a reaction, we should have to consider the retinal stimulus in its own size-properties or the body far away which lies "behind" it as its partial cause and is represented by the stimulus to the organism. The problem would be, in short, whether an organism "sees" the retina or the remote environmental bodies.

We find an objective basis for deciding questions of that kind in constancy-research. Since the findings in higher animals did not afford a constant coupling between retinal projective size and reaction, but a rather satisfactory one between body-size and reaction, we may call body-size (and not retinal size) the attained object of reception. In the same way, under normal conditions, not the stimulus-intensity of arriving light-rays, but the reflectivity of body-surfaces would have to be called the attained object of color-reception, and likewise note the intensity of the received, but of the emitted sound the object of a normal auditory reaction, etc. This way of experimenting upon and describing an individual's abilities by *projecting the reactions upon their focal conditions*, or upon the environmental end-terms of the (cognitive) couplings, we may call "*psychology in terms of objects*" (*Psychologie vom Gegenstand her*).[11]

[11] Our emphasis upon the "object attained" may be considered as a kind of *long-sectional figure-ground treatment* of psychological research following the stimulating causal chains backwards in search for their actual "meaning" (see above), i.e. for those types of object within the environmental system upon which the reaction became focalized. The relationship of the "object attained" to "mediation" is a complement to the more cross-sectional relationship between figure and ground as emphasized in Gestalt psychology. Ground as well as mediation are both characterized as being present as stimuli but remaining lost amidst the "things" which they are "framing." *Cf.* also F. Heider, "Ding und Medium," *Symposium*, 1927, 1.

As can be shown, this method would not be limited in its application to the psychology of reception, but could be extended equally well to the psychology of overt action. In the former case the causal chains entering the organism are followed systematically backward until they reach types of causes which prove themselves to be, at least in "normal" cases, the most essential focal causes for the outcoming reaction; whereas in the latter case the causal chains set into action by some movements of the organism would have to be followed in a great number of different cases and under systematically varying conditions in forward direction in order to find out the common ends into which they converge (unless they are disturbed by "extraordinary" conditions). In studying action problems, psychology in terms of objects would specify a discipline which would operate in terms of success.

In problems of reception (i.e. perception and thinking) as well as of action, psychology in terms of objects would turn out to be a physical and biological natural science, being concerned in particular with all kinds of fairly well-established (i.e. fairly univocal), far-reaching, interruptable causal couplings between the classes of reactions on the one hand, and the corresponding classes of releasing, or effected, "attained" types of (environmental) constants or events on the other. *This—rather than the intrinsic properties of behavior as such—seems to me the primary topic of psychology, which thus appears to be defined by a formal criterion, as a certain particular type of objective correlation.*

The essentials of all response or behavior would in this way be projected upon the total manifoldness of the physical environment, segregating from it the particular "world" of the individual (or of one of its sub-functions, as, e.g., perception) as a part of the intellectually constructed world of a highly sophisticated human physicist. This would be "his world," the "*Umwelt*" (to borrow this term from Uexküll) for which he was able to establish fairly reliable cues and means, and which he thus mastered in cognition or in action (or in both at the same time).

This reacted-to world could be detected and described in a purely objective fashion. In fact, constancy research can be and

has been undertaken equally well with animals as with human beings and has shown up highly developed achievements of thing-constancy in some of them. The general method may, in the representative case, be guided by the following frame: (1) a search for all equalities in the field of reactions, (2) a registration of all traits of the corresponding environmental situations, and (3) a finding out of those traits among them which are equal when reactions are equal. This is, in short, the method of reactional equalities, or of equivalent situations.[12] The particular emphasis from the standpoint of psychology in terms of objects lies on step (2), requiring a *satisfactorily abstract and differentiated conceptual system of possible objects* (see below).

Furthermore, constancy research is not limited to the organisms's environment. As the work of physiologists like Cannon shows, even the "wisdom of the body" may be expressed in terms of its established constants, as e.g. in terms of the physiological regularities of temperature, blood-composition, etc., which all appear to be kept in a high degree invariant independently of a great number of randomly varying circumstances.

Thus the topic of psychology in terms of objects would be all kinds of "constant" couplings—kept fairly undisturbed from interfering "lateral" causal chains—between separate layers of the environment and of the organism, or within the organism itself, or even within the environment; all of them would, in fact, require the integrative functioning of an organism or of one of its tools (as e.g., machines or collecting lenses).

Let us compare, in short, psychology in terms of objects with some of the outstanding traditional forms of psychological investigation: the early behavioristic research and the traditional type of psychophysics.

Leaving out of consideration some generalities not sufficiently emphasized to become effective in research or in detailed conceptual systematization, in both of these fields a rather undifferentiated stimulus-reaction scheme was used, which did not even sufficiently keep in mind the distinction between proximal stimuli

[12] *Cf.* also H. Klüver, "*Behavior Mechanisms in Monkeys*," Chicago 1933. Some of the work of Lashley follows the same principle.

and distant things or between reaction and its distant results. Since, e.g., in size-comparison experiments both bodies—the standard and the variable—were put carefully at the same distance from the observer and in the same surrounding, no decision could be made regarding our alternative mentioned above, namely whether retinal size (visual angle) or body-size is the actual object of perception. The outcome of this type of psychophysical experiment was, therefore, not much more than data about thresholds, i.e. the mere sensual acuity under certain very favorable but specified and artificial conditions, and not of the avoidance (or non-avoidance) of gross errors impending under the disturbing and misleading conditions essential to the random variability of practical daily-life situations. The *kind* of object actually mastered can not be disclosed unless the disordered variations of the proximal stimulus representations of the environmental entities have received a proper place in experimental research. On the contrary full emphasis is given to the neglected questions of the disturbances involved in these variations, in constancy research, where objects set up at different distances, indifferent orientations, surroundings, etc., are to be compared intuitively.

In a way analogous to psychophysics, the early behaviorism and physiological psychology seemed to become overconcerned with the mere mediation-problems of sensory, nervous and muscular action as such, and therefore lost contact with the decisive more remote focal causes and effects of these actions which have to be searched for in regions of the environment spatially or temporally distant from the organism in its present status.[13] Psychology appeared to be almost limited to events in the organism itself or on its surface, whereas the anchor-points of life lying outside the skin did not enjoy an equal amount of analytical care.

[13] Even G. F. Skinner ("The Concept of Reflex in the Description of Behavior," *J. Gen. Psychol.*, 1931, 5, 427-458), though maintaining that "psychologists had better give up the nervous system and confine their attention to the end-terms, does apologize for doing so in pointing toward the greater immediacy of observation of these end-terms and the reduced temptation for insecure speculation. We do not find it a matter of embarrassment, but rather one of positive emphasis, to go even further in psychology and restrain—as far as at least one term of the correlations in question is concerned—completely from the organismic events as such in favor of the initial (or terminal) environmental limits or focal systems as connected with one of the organism's activities.

Psychology in terms of objects, on the other hand, tries to apply some method which could be called *object-critical* (*gegenstandskritisch*): the type of object really attained in reception or action is ascertained by the application of objective operations and described in its precise conceptual distinction, involving a widespread conceptual differentiation of the older comprehensive notion of the "stimulus." The "*what-problems*" of objects attained are put in the first row and the "*how-problems*" of mediation admitted only in so far in psychology as they throw light upon what-problems.

II

The psychological problems treated in section I were apt to be formulated in an objective, behavioristic fashion. An organism was supposed to be studied in its causal relationships to the environment by a neutral observer in the same way as any physical problem would have been treated. If the proposition is true that we were concerned with a scheme for the most fundamental problems in psychology, we must be able to answer the question as to what bearing these considerations may have to that field which for a long time has been considered the central or even the only one in psychology: consciousness.

The problem of perception in the traditional type of psychology-psychology of consciousness—is usually stated in terms of antitheses like the following: consciousness vs. reality; appearance vs. existence; subjectivity vs. objectivity; realm of immediately experienced introspected phenomenal qualities vs. realm of constructively inferred physical quantities; etc. In those terms the facts of thing-constancy at once appear in a fashion more familiar to common sense: a complete perceptual constancy would simply mean that a real 8-cm. body under all varying circumstances always succeeds in appearing in the apparent size of 8 cm., or a white chalk under all illumination conditions as white, etc., or, in short, that bodies would appear always in the same way as they really are, and that always what seems to be equal, is equal and vice versa. These formulations seem at first to touch many problems and differentiations usually treated in philosophy and not

in an empirical science. But since our considerations in the previous chapter have shown us that an objective formulation of the constancy problem does not involve any metaphysical complications, we may be still hopeful that even the introspective version of our problem might be formulated in a way which is, and which will even look, philosophically neutral.

As a basis for physical statements not all kinds of observations are equally admitted. As Schlick[14] has especially emphasized, a certain type of observation plays an outstanding rôle as a basis of physical judgments: the observation of complete spatio-temporal coincidences. All kinds of "*measurements*" rely upon this highly specialized type of observation.[15] The main outstanding feature of this type of observation is an ideal self-consistency (non-variability) of the data, which makes it possible to build up out of them a "world" which is free of contradictions. However, no philosophical distinction between an ordinary and such a selected type of perception could be made. Physics and any "topographical" description of the world are, primarily, a constructive outgrowth of a set of measurements. For the introspecting psychologist there is no need to discuss any further questions as to an "independent reality" corresponding to this constructed physical world. He may for his own empirical purposes stop at this point. The problem of perception, therefore, would appear as a comparison of the results of the more ordinary, carefree and everyday-life observation—called perception in a narrower sense of the word—on the one hand, with the outcome of a critically selected special set of observations followed by processes of explicit discursive intellectual and conceptual construction—i.e. the "physical" world of measurement and computation—on the other hand. Thing-constancy, then, could be formulated as a good correspondence between intuitive judgment and measurement of the characteristics of the environmental bodies, regardless of the varying circumstances under which the intuitive judgments occur.

[14] *Cf.* M. Schlick, *Allgemeine Erkenntnislehre*, Berlin, Springer, 1925.

[15] As it has been pointed out above, coincidence may be used sometimes as a help to attain explicitly in perception a certain kind of object which otherwise would not be represented by a conscious dictum, e.g. the retinal stimulus-sizes.

In a formal sense the observer of a complete coincidence underlying the operation of measurement is an observer of a relational datum, in particular of an equality. No "qualitative" statement is involved. It is this trait which yields to physics its "objectivity," that is the highest possible degree of self-consistency and unambiguous social communicability. This standard was adopted for psychology in section I, referring to the "method of reactional equalities." "Consciousness-psychology" may equally well fulfill this ideal. *Excluding all direct metaphorical reference to conscious "contents" we may restrict to the subject's report to equalities among his intuitive contents,*[16] *and yet get hold of the most subtle phenomenal differentiations, or qualities, by "projecting" them upon the corresponding "objects attained"* and thus representing them in environmental terms. This can be done by means of an analysis of the releasing situations involved—thus leading, in the end, to a *fully appropriate analytical treatment of totalitarian problems.*

As an outstanding trait of these reactional equalities there remains their reduced self-consistency and high variability in terms of their objects attained. This may be ultimately due to the fact that, like under variable atmospheric conditions, there is a steady interference with all sorts of coinciding outer and inner inequalities. For example, surfaces perceptually recognized to possess a certain well-known "color" (reflectivity) will be different from each other in space and time, will stay under different illumination, in different surroundings, have different areas, etc., and thus never remain quite untouched in their appearance.[17]

[16] The author, *l.c.*, tried to show that even the mere *naming* of the apparent equality in question in conceptual terms—i.e. by indicating whether it is an equality of apparent size, or color, or weight, or density, etc.—is, in principle, omitted from "psychology in terms of objects."

Not only the actual attainment but even the types of intentional effort or "attitude" toward attaining certain "intended" objects may be disclosed by objective methods (in fact, by analyzing the statistical distribution of judgments with regard to the number of modes).

[17] I agree with H. L. Hollingworth ("Experimental studies in judgment," *Arch. Psychol.*, 1913, 29), who proposes, in accordance with Wells, to use variability as an objective, quantitative index of "subjectivity" of judgment. (I would not, however, as Hollingworth does, rely upon a ratio of personal vs. group consistency, but keep the definition free from its "social" element and emphasize the more general point of non-attainment, or variable attainment, of a type of object.)

But, likewise as is true for the mind, this would not involve an obstacle to an exact treatment. Spatio-temporal point-coincidences as a selected basis for physics is a simultaneous equality of two elements in *all* possible respects. That is the trait which makes it especially outstanding.

Both versions of our problem, the behavioristic as well as the introspective, appear to be in an equal way *philosophically neutral*, or non-metaphysical, since no change in the universe of discourse or general categorical system involved is necessary. The introspective version need not transcend the "realm of consciousness," since the problem may be kept restricted to intuitive, set off against sophisticated conscious data. The behavioristic version, on the other hand, will play, in principle, within the frame of these sophisticated, constructive data alone, confronting measured reactions of an organism with measured properties of its surroundings. These two versions may be considered ultimately synonymous, or tautological, thus giving but two different aspects of one and the same empirical problem.

An interesting and promising statement concerning the subject-matter of psychology was made by Brentano.[18] He considered it essential for anything psychic "to have an object": the subjective contents of perception do "mean" something, which is distinguishable from them, as their object; so, for instance, my impressions of the table are pointing toward the real table. When we love, hate or fear, we love, hate or fear something; and so on for all conscious contents. This relation of the given contents to certain objects has been called "intentionality." Thus the real table would be the "intentional object" of the corresponding perceptual content.

Having in this way started a fruitful point of view, Brentano in some other respects failed to meet the decisive empirical questions, as formulated in the constancy problem. He was troubled by something he called different ontological status or "way of existence" (*Seinsweise*) of content and object, the former being subjective and phenomenal, the latter objective and real. Therefore,

[18] F. Brentano, *Psychologie vom empirischen Standpunkt*, Leipzig, 1874.

he hesitated to call intentionality (or "meaning") a genuine relation, since a relation may not link together entities which in some essential respect are incomparable. Meaning or intentionality should, according to Brentano, rather be called something (quasi-relational ("*ein Relativliches*"), and in any case has to be considered as a relationship of its own original type, which by no means could be analysed with regard to its properties or be reduced to any other type of relation. Many other philosophers have followed Brentano in this point.

Brentano entitled his main work "Psychologie vom empirischen Standpunkt," but his notion of what is empirical—not very dissimilar from that of other "act-psychologists" and "phenomenologists" (like Meinong or Husserl)— was a quite introspective one: the actual object of any "act," or of its "content," has to be found by an immanent phenomenological analysis of this content itself. What the object of my perception is, may be read from this conscious phenomenon itself. It seems to me that this definition of the concept of "object" is a good example of what has been called "unnecessary duplication" of the world into phenomenon and reality. If the object of a perception is completely clear from an analysis of that content which is considered merely to represent it, there is no forceful reason to separate these two entities at all. Corresponding to his (partially) wrong beginning Brentano's work lost itself in a kind of dogmatic phenomenalism which did not yield useful empirical results.

As may be seen from the former considerations, the present writer would agree with Brentano in defining the subject-matter of psychology by the *bipolar, diadic concept of intentionality*. But we would maintain a further step of development of this concept beyond act-psychology, abandoning the immanent-phenomenalistic trait in its definition. In order to get in touch with exact work in psychology, an *objective re-definition of intentionality and an indirect method of detecting the actual object* of any content has already been outlined above. For the sake of convenience, the couplings found in constancy-research would be called "intentional," indicating the life-sustaining effects of their establishment and their functional pointing towards something present in

the environment.[19] Instead of the older notion of an "intentional *in*-existence" of the "intentional object" in the (conscious) response we might, then, better talk of an "intentional *co*-existence" of the object attained with the response.

In order to discriminate from Brentano's "*intentional objects*," those types of physical objects for which perceptual constancy holds should be called "intentionally attained" objects. Psychology in terms of objects would, then, as far as problems of cognition are concerned, not be a description of the terminal limits of the "intentional causal couplings," i.e. of the organism's reactions (contents), in terms of properties of their own or of their "immanent intentional objects," but a description of the initial limits of these couplings, i.e. of the organism's actual cognitive achievements, in terms of the "*objects intentionally attained.*" (To indicate an effort made towards the attainment of an object this object may be called "*intended.*")

The statement about the nature of the actual object of any reacting organism or observer (subject) could not any longer be made by the subject himself on the basis of an immanent introspective analysis of his actual content as such, but would be made by the experimenter—or at least by the subject in a changed rôle with additional empirical measurement support—on the basis of the outcome of an experiment, and therefore would gain objectivity. In cases where, by means of these indirect methods, equal reactions (or phenomena) should be found to correspond with equalities in certain (retinal) stimulus-values, these proximal stimuli would have to be called the intentionally attained object of perception; whereas, if reactional equalities should correspond to equalities of the distant bodies these bodies (or their abstract physical characteristics) would be called intentionally obtained objects. In fact, the outcome of such an object-critical analysis could be repeated, and therefore would have to be acknowledged, by everybody, just like the outcome of any other physical experiment.[20]

[19] Thus the new concept of intentionality seems to be closely related to what Tolman meant in defining in objective terms the "purposive" character of behavior.

[20] For a concrete example of the difference between the phenomenological and the object-critical method of object-finding see page 246.

The revised operational definition of intentionality as an objective diadic relationship between individual and environment would also abolish its irreducibility and make it capable of a logical analysis in terms of the general relational theory as outlined by Whitehead and Russell.[21] According to our previous analysis of the couplings involved (see section I), the ideally perfect intentional relation would possess the character of a one-to-one (i.e. reversibly univocal), interruptable (i.e. multiply and variably mediated) causal relationship.

Furthermore, the intentional relationship shows some similarity with the marital relationship between husband and wife. Though this relationship is in both directions univocal—since each husband may have only one wife and each wife only one husband—it is yet "*asymmetrical*": the wife of the husband is not the husband of the wife. This attaches to the relationship a certain direction. In the same way it will hold for an intentional coupling—at least so long as it proves to be an ideal one—that, e.g., an 8-cm. stick will under all conditions look like 8 cm. long and anything that looks like 8 cm. long will also be found in measurement to have this particular length. But nevertheless reaction and object are not interchangeable, their relation possesses a certain direction, it is "asymmetrical."[22]

Finally, the objectively defined intentional causal coupling—besides being diadic, reversibly univocal, "interruptable" and asymmetric—would possess a certain kind of *transitivity*. For, all objects staying in a univocal relationship to a type of object which is intentionally attained could be considered even themselves as intentionally attained. This feature would become especially important in all kinds of "understanding" of representation of facts by the means of language or any type of "expression." The logical traits mentioned yield a complete analytical concep-

[21] B. Russell, *Introduction to Mathematical Philosophy*, New York, Macmillan, 1919.

[22] The intentional relation thereby might be defined as positive either in the direction from the object as initial term to the reaction as terminal limit—following the direction of the causal chains—, or in the opposite direction, from the reaction to the object as end-term. This latter would be more in accordance with the character of the perceptual reaction as a preparation to overt action towards the body in question as a manipulable means-object, and would also follow the direction of the immediately experienced "meaning" or conscious intentionality of the Brentano-type.

tual reduction of the operationally re-defined concept of "intentionality."

III

Constancy-research, or the investigation of intentional couplings, is concerned with practical achievements of living beings. Therefore, it may not be expected to find as ideal results as have been assumed in the previous chapters. In fact, empirical investigations showed certain characteristic deviations from univocality which need for their precise representation a further differentiation of the conceptual system of objects in order to make possible a description of the achieved relationships in terms of their object-ends.

A simple size-constancy experiment is the following. A cube of 8 cm. height is set up in front of the observer at 1 m. distance. At 6 m. distance a variable series of cubes can be presented. Under usual everyday circumstances, in the average, not an 8 cm. but an $8\frac{1}{2}$ cm. cube will be found to be the apparent equivalent to the standard 8 cm. cube in front. This can be considered as a slight, but nevertheless noticeable, influence of the diminished visual angle under which the cube far away is projected on the retina.

On the other hand, the objective projective equivalent of the standard cube in the plane of the comparison series would be a 48 cm. cube. But under usual conditions (excluding the case of retinal coincidence as mentioned above) even in the case of a strong effort toward an antagonistic analytic ("pictorial") attitude, an observer, not especially trained, will fall far short of attaining this value. Let us assume—in accordance with experimental findings—that the apparent projective equivalent will lie around 12 cm. In terms of an achievement analysis, this result indicates the inability of the perceptual system to get away in a satisfactory degree from its deep-rooted habit of bodysize-constancy; the deviation of the apparent value (12) from the true (48) has to be accounted for, again, as a mutual interference of the two different viewpoints of comparison: body size and projective size.

What perception really attains in both cases is a kind of compromise between two different types of "objects." In no case is the gross perceptual efficiency following an all-or-none principle in terms of relationships to environmental entities. But in the former case the attainment of the "intended" type of object is obviously much closer to the aspiration than in the latter, since $8\frac{1}{2}$ is nearer to 8 than is 12 to 48.

An exact quantification, in terms of objects, of the "*degree*" *of bodysize-constancy* in both cases can easily be afforded by introducing a new scale which has its zero point at the value where not even a slight deviation away from a correct retinal comparison —which is, from a biological standpoint, a cheap but "bad" truth—towards size constancy would be realized, that is, in our example, at 48 cm. The point of unity, or of a "100% constancy," would, in our example, correspond with 8 cm.[23] For different reasons, not to be discussed here, the scale has to be subdivided in a logarithmic manner. According to a simple "constancy-ratio,"[24] the location of $8\frac{1}{2}$ and 12 on this scale would then be computed as .96 and .76 respectively indicating a remarkably high bodysize-constancy in the former case, guided by an intentional attitude towards it, and a respectable remainder in the latter case, where the individual made an opposite intentional effort.

A similar general way of response, but usually with less high degrees of constancy, has been found in the field of color constancy as well as in gestalt-constancy. The apparent size of the moon also follows the same principle of perceptual compromise. It has been found, furthermore, that the attainment of both types of objects—body-properties as well as retinal stimulus values—will increase slowly but steadily with repeated performance of the task, thus increasing the shift-span between the achievements of the two antagonistic perceptual attitudes (or modes or evaluation of the stimulus-configuration) in question.

[23] This scale could equally well also have its ends mutually exchanged.

[24] *Cf.* E. Brunswik, Zur Entwicklung der Albedowahrnehmung, *Zeitschr. f. Psychol.*, 1928, **109**, 40–115, and *l.c.* The degree of failure with respect to body-size is indicated by deviations either below or above 100.—*Cf.* also R. H. Thouless, Phenomenal Regression to the "Real" Object, I, *Brit. J. Psychol.*, 1931, **21**, 339–359.

The problem of perceptual constancy is not limited to alternatives of "independent" body-properties vs. projective stimulation properties. It can be generalized and transferred to perceptual "confrontations" and various other kinds of objects.

Let us take, e.g., a standard body weighing 15 grams and compare it with a set of comparison bodies of varying weight but each of them of equal volume, which might be twice the volume of the standard. Among this comparison series, therefore, the 15 gram object will be equal in weight to the standard, but different in density, whereas the 30 gram object will be equal to it in density and not equal in weight. The question put before the perceptual system is quite analogous to the alternatives discussed above. Perception has to decide whether it will acknowledge the type of equality holding between the standard and the 15 gram comparison-object; then we would have to draw the conclusion that "weight" was its intentionally attained object. Or perception may decide for a reactional recognition of the type of equality holding between the standard and the 30 gram object; then "density" has been the object of perception.

As could already be expected from the outcome of the other types of constancy experiments, perception even in the case of the new alternative—weight vs. density—does not yield responses which could be called "clean" (or all-or-none) in terms of the object attained. Neither weight- nor density-constancy has been found ideal under the conditions under which the experiment was performed. As has been found by Izzet, *l.c.*, in average, instead of the 15 gram object, the 17 gram object has been chosen as apparent weight-equivalent; and instead of the 30 gram, the 21 gram object as apparent density equivalent. Therefore, the same type of interference as above could be found again between different kinds of physical objects even in weight- and density-perception, showing a weight-constancy of about .80 and a density-constancy of about .50.[25]

[25] In a quite analogous way, in experiments with figures, interferences have been found between area and length of the edges, etc. (see below).

Some of these interferences, especially those occurring in the field of the original type of constancy research (as size-constancy, etc.), may be satisfactorily accounted

It may again be pointed out that a traditional psychophysical weight-threshold experiment would not allow such a decision as to the kind of object actually perceived, since volumes would all be kept equal, and therefore the objective weight- and density-equivalents would coincide. It would leave open the question of "what" we perceive and give merely an account of the precision of perception as a measuring device under certain especially favorable conditions.

What should properly be called the intentionally attained object in cases like those mentioned, where neither the one nor the other correct "*pole*" of intention, i.e. no perfect constancy, is attained? An answer from the standpoint of an either-or would be: none; but this would not seem very reasonable, considering the clear-cut nature of the empirical results. The right proposition to make seems to be the following: In cases of interference or compromise of two (or more) different kinds of possible viewpoints of comparison, let us call an "*in-between object*" (*Zwischengegenstand*) the object of perception, as determined quantitatively by the constancy-ratio mentioned above.

The concept of an in-between object might at first seem paradoxical from the standpoint of the usual system of objects. In

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for *in terms of the causal texture of the environment in its relationships to the organism.* Perceptual cues for remote objects always remain ambiguous, and therefore perception is forced to co-include in its basis for reaction a large number of more or less indirect and unreliable cues which in some cases may stay in but a very low correlation to the type of object which they are admitted to indicate. (This would hold, e.g., for retinal size *per se*, when taken as indicating body size. Indeed, the characteristic odd admixtures found in the results of size-constancy experiments may be represented as functions of the product retinal size times indicated distance [i.e. body-size], on the one hand, and of retinal size, *per se*, on the other.) As has been pointed out in the author's article "Psychology in Terms of Objects" (*l.c.*), the effect of such a general way of functioning is a decrease in the probability of exceedingly large perceptual errors.

For some other types of perceptual compromises an explanation *in terms of the technicalities of the sense-organs* at the command of the perceptual system might be found, by studying physiologically their structure and the way of their functioning. This seems to be especially true for pitch and loudness which both turned out to be joint functions frequency and energy of the sound. *Cf.* S. S. Stevens, "The relation of pitch to intensity," *J. Acoust. Soc. Amer.*, 1935, **6**, 150–154, and E. G. Boring, "The Relation of the Attributes of Sensation to the Dimensions of the Stimulus," *Philos. of Science*, 1935, **2**, 236–245.

physics something may be either a weight or a density, in geometry either a length or an area, etc., but never a hybrid between two or more of these essentially different and in some way incompatible entities of different geometrical or physical dimension. But we are easily able to find analogies from the field of mathematics. The introduction of the complex numbers $(a + bi)$—where $i = \sqrt{-1}$—is filling up the empty field between the two orthogonal axes constituted by the scales of the real numbers on the one hand and the imaginary on the other, in the same way, as the logical constructs of the in-between objects do with the open gaps within the discrete system of the sharply distinct abstract types of objects, used in physics and geometry, into a continuous system of objects. These constructs are in no fundamental way different from the constructs used in other sciences. And they are also not mere products of speculative fancy, but means for a convenient quantitative representation of the essentials in the results of an empirical psychology, and therefore an outgrowth of a real scientific need.[26] In fact, they are nothing but a short means of expression of the finding that the type of perceptual response in question is a function of more than one of the variables as they are conceptualized in the current system of physics and geometry.[27]

[26] Some relationships could also be found with the manivalued logics, in which the absolute alternative of true and false is given up in favor of a continuous scale between these two cases as mere extremes.

[27] It could be objected that a presentation of psychological results in terms of functional dependence of the reaction on various stimulus-factors would be as short and less confusing than to talk in terms of "in-between objects." The reason why we believe it more profitable, however, to make this terminological distinction is the following. Strict functional dependence would have to be expressed in terms of proximal stimulation. We would prefer, instead, to *express the (functional) relationship in terms of the remote significate*, instead of using the signifying stimulus, in all cases in which the operational criterion of meaning given in section I is fulfilled. This would necessarily introduce a certain ambiguity. But it may be still considered more favorable to do so from the standpoint of illuminating the essential cue-rôle of the stimuli in establishing the organism's ability to master its environment.

Let us consider an example. For a most direct functional analysis apparent size would be simply one or another function of retinal size and distance-cues, and the case of a perfect constancy would not be especially emphasized as against cases of incomplete achievement. In terms of "objects attained," however, the response would appear to be a "function" of body-size alone (i.e., in strict functional terms, of retinal size times

By an extension of the conditions varied, the reaction has been proved in certain fields to be a function of more than two types of objects, as, e.g., in the case of a comparison of volumes in different forms, or in cases when the numbers of groups of dots, coins, or stamps are to be judged intuitively. In this latter case the size and even the value of the elements will influence the apparent number.

Precise statements as to the minimum number of abstract physical factors ("objects") which have to be considered to participate in the in-between object attained in any particular type of perception will, it is hoped, be available by some application of the mathematical methods used in factor-analysis. The aim would be a *generalized 'multipolar," or "multidimensional," psychophysics*, which would enable us to register the structure of the world as attained by the organism in an exact quantitative inventory.[28]

After an introspective analysis, Katz[29] distinguished two main "modes of appearance" of colors. Either a color looks compact and substantial, as belonging to a solid thing, in which case it appears sharply localized (e.g. at the surface of a paper), or it appears somewhat unreal, a mere sensational "color matter" without reference to a thing, with a spongy texture and not definitely localized in its distance. The colors as seen in a spectro-

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indicated distance) in the case of a perfect achievement, and of body size and projective size, *per se*, in the case of an incomplete achievement. In the latter case retinal size would enter the function twice in different rôles, once as a constituent of body-size, then *"per se"* (see the note above).

A further complication for a functional analysis in terms of retinal stimulation seems to be that all equi-potential members of a cue-family (see above), e.g. all stimuli and stimulus-configurations indicating distance, would have to be enumerated explicitly in terms of intrinsic properties of their own instead of simply being comprehensible in terms of their common "significate," as "distance-cues." By using the "object"-terminology the particular kind of environmental-functional direction of interest in stating the dependences found could be indicated at once.

[28] Since the ways of mediation will always determine the achievement, the highly abstracted type of object-critical analysis as outlined above would lead, ultimately, to a statement of all psychologically relevant types of "how"-problems and -findings in terms of "what," i.e. of objects attained.

[29] D. Katz, *The World of Color*, London, P. Kegan, 1935.

scope, or the color of a hole in a screen, would appear in this latter way. The first mode of appearance is called "surface-color," the second "film-color."

The appearance of a surface-color would, when applied to Brentano's idea of intentionality, indicate that the (color of the) solid body remote in space is our intentional object. A film-color would represent the case in which no "thing" is supposed to be our object, and which, therefore, sometimes has been called the case of a "non-intentional" experience or of "mere sensation." The vagueness and indistinctness of the phenomenological method of determining the object of perception shows itself, among others, first in the fact that no precisely communicable treatment and no detailed quantification is possible in that kind of analysis.

Similar distinctions to those of Katz were made by Holaday (*l.c.*) and by Eissler (*l.c.*) as to the appearance of sizes and forms: under clear conditions they will appear "thingish" (*dinghaft*), under unfavorable ones in a more ghostlike and essenceless, merely "*figural*" way. Independently of that type of analysis, the main task of transferring of the procedure of object-finding into an objective plane was achieved by conducting constancy-experiments. Having done this, a correlation was found between the "thingish" mode of appearance and high degrees of thing-constancy, on the one hand, and the "figural" appearance and lower degrees of thing-constancy, on the other. But nevertheless the correspondence, as far as it could be ascertained at all, did not seem to be a very close one, showing that immediately experienced intentionality is a rather unreliable indicator for the goodness of perceptual achievement of a type of object in question.

IV

Let us, finally, turn to some applications of the psychology in terms of objects to current, more general questions. It is a frequently repeated statement that the distinction between "immediate experience" and "reality" is an undue duplication of the word. The duality in question is supposed to be merely one of representation. All datum might be described either immediately in terms of qualities or mediatively in quantitative terms. So,

e.g., for "red" a certain range of wave-lengths could be substituted, for "experienced weight" the outcome of a weight-measurement, for "visual space" physical space, etc.[30] Both "languages" —it is supposed—can be easily translated into each other and indicate the same thing.

From the standpoint of constancy research, the following objection could be made against this statement. A projection of the immediately given world upon the scientific system of objects does not show a complete parallelism. The representation of the intuitive qualities within the abstract world of objects comes to lie in the empty spaces between the "clean" objects of physics and geometry. Therefore the lines coordinating the two aggregates in question to each other do not show a parallel and unambiguous texture but a complex entanglement. Therefore it is not "the same" (or "nothing but") that is represented once in qualitative and then in quantitative terms, when we describe e.g. the Eddingtonian table once as apparently so and so big, rectangular and black, and then in the abstract measurement terms of physics. As was pointed out above, the intentionally attained object of apparent body-size is not the measured body size, but an "in-between object" between it and retinal size; and the same holds for apparent vs. measured form, color, area, volume, weight, number, etc.

Under this aspect the "duplication" of the world into an "immediately given" and a reflectively constructed one does not seem any more undue or unnecessary, since their confrontation turned out to be a sincere empirical question, which in each particular case can be decided by experiment. It is true that intuitive perception gives a portrayal of the physical world, but this portrayal is in a certain way imperfect, not completely isomorphic; an investigation of this incompleteness is the very task of psychology.

Therefore, the "intentional," psycho-physical problem which we are dealing with can not be considered to be a mere pseudo-problem, an outgrowth of mere conceptual confusion. From the point of view of an achievement-analysis, the intuitive and dis-

[30] As our examples show, we are dealing here with the "psycho-*physical*," not with the "psycho-*physiological*" relationship.

cursive approach to the world do not have the same objects, and all their differences, as well as all differences in qualities, can, ultimately, be expressed in terms of their differences in objects attained. And though these differences are often minute and subtle, they will never fundamentally disappear. They give sufficient support for the type of "dualism" suggested. This dualism at the same time lost all its philosophical disturbances, since at no point of our underlying psychological consideration was any metaphysical question involved. It became a dualism of quick, carefree, stereotyped, intuitive experience vs. a critically selected and logically treated experience. The absolute gap between these two layers disappeared, but the difference became the more clear in quantitative terms of objects. This dualism may be considered to be the harmless and necessary heir of at least some aspects of the historical, unnecessarily troublesome dualism of "mind and body" or however else it has been formulated. It does away with the naive presupposition widely accepted by common sense that each experiential datum refers to but one of the scientifically conceptualized variables of the stimulation-process.

Let us turn to a second point of consideration. Empirical philosophy made much effort in order to find an "*indubitable*" basis of facts. A regress to the phenomena as given in completely naive immediate experience was suggested. Mach considered the mass of sensational elements as primary, Russell the more unitary "aspects" of things. These aspects have been considered to change with the position of the observer toward them. The thing itself would be a sort of collection of all of its different aspects.

From the standpoint of constancy research, it may be objected that both Mach and Russell were still not naive enough in their description of the immediately given. According to the experimental findings, a "thing" practically does not change its appearance with changes in distance, orientation, or illumination, etc., or at least by far not in the same degree as its proximal stimulus-representation does. A table remains, just in its most naive and immediate appearance, under all conditions the same phenomenally identical table as ever; its backside or its weight is phenome-

nally as present as its frontside or its color, no matter whether these phenomena correspond to "truth" (measurement) or not.

The incorrectness committed by Mach and Russell obviously has its causes in an incomplete elimination of functional considerations from phenomenal description (a widespread source of confusion, sharply critized by Koffka); this was meant above by using the term: incomplete naiveté. In a way similar to their contemporary psychology they did, in essence, unduly overemphasize and absolutize "the skin," i.e. the structure of and the stimulating events on the retina and other stimulation-surfaces. The notion of the sensational elements of Mach cannot belie its descent from the punctiform mosaic structure of the sense-receptors in the retina or other parts of the body. And the "aspects" of Russell correspond to the direct projective patterns reaching the stimulus-surface. The main difference between Mach and Russell is that Russell had in some sense been infiltrated with a totalitarian Gestalt point of view, whereas Mach—though as an empirical scientist himself one of the initiators of the Gestalt movement and of constancy research—remained a psychological atomist in his principal theoretical ideas.

Psychology in terms of objects (which goes, in some respect, a further step beyond the old elementistic sensation psychology as well as beyond Gestaltpsychology) has to stress even more than it has been done before, the relativity of the concept of the immediately given (or the undubitable). Through more or less arbitrary shifts in attitude, a great variety of experiences can become phenomenally indubitable and primary. This was shown in detail by several experimental studies. Mach's famous unique juvenile experience of the world as being "genuinely" nothing but a mosaic of color, taste and smell, is a type of experience which never will happen except in a very extraordinary kind of analytical attitude.

From the standpoint of an achievement-analysis of the organism, in last essence, no fundamental distinction can be drawn between "immediate" and "discursified" responses. From this functional viewpoint it would not even be true that the physical world contains inference, whereas the phenomenal not. The facts

of thing-constancy—which of course are deeply concerned with the immediately given—could never be accounted for in functional terms without the assumption, most fundamental in psychology, that stimuli of various kinds are used as indicators for something else which causally lies "behind" them (*cf.* our operational criterion of meaning given above).

The formation of such quasi-"hypotheses" within the perceptual system has been studied repeatedly. Stratton was the first to make perception relearn the use of retinal images by turning them upside down through the application of certain lenses in front of the eyes. In recent experiments, the acquirement of artificial new cues by the perceptual system has been studied. Fieandt, in a study which is to appear soon (*l.c.*), succeeded in training perception to use in an intuitive way certain otherwise meaningless stimuli—e.g. even the ringing of a buzzer—as cues indicating the illumination-conditions, in particular a certain area of shadow carefully hidden from the somewhat narrow system of well-established perceptual shadow-cues. In consequence the introduction of this stimulus was followed by a quite immediately given apparent brightening of a certain shadowed disc, establishing its color constancy.[31]

But, nevertheless, all these training effects did not appear in the same sudden and radical way as discursive information would have affected our intellectual knowledge. On the contrary, they needed long-repeated training, and grew up but slowly and incompletely. The brightening effects of the newly established

[31] It has been found that even *ambiguous cues* (i.e. those of a reliability of their actual indicative value lower than 1) will be conditioned, though the response seems to retain a higher degree of tentativeness (reduced strength). In a recent unpublished study of the present author, conducted in order to throw further light upon this problem, rats were confronted with objectively ambiguous situations, possessing various degrees of probability of success or of punishment ("danger"). Even relatively small differences in probability were discriminated by these animals (whose functioning is, in fact, not very dissimilar to the relatively primitive cognitive system, called perception). As the writer hopes to be able to show later in detail, an evaluation of even some of the slightest correlations between possible cues (symptoms) and more remote traits seems to be true for perception, especially in making accessible the character-traits of other persons in social intercourse. Reactions to mere probability have also been found by Thorndike in some of his recent learning experiments.

shadow-cues did not seem to be affected by the introduction or the lack of any direct "conscious" recognition of the particular stimulus as an indicator for the fact that the disc is shadowed. Similar (relative) functional isolation of perception from abstract knowledge is familiar from many of the well-known "illusions."

The same relative independence from discoursive knowledge, and indication for the same more mechanical way of learning, was found in experiments with coins and stamps, where the influence of "value" upon the apparent number did not exceed a certain degree unless a sufficiently deep-rooted "underground" familiarity with the objects as bearers of value was established, even if this rôle was discoursively well-known to the subject. On the other hand, these experiments showed that even "unsensual" and "conventional" traits like monetary value may enter the system of perceptual intuition.

In a merely functional sense, therefore, intuitive perception seems to be a somewhat autonomous but more primitive cognitive function (or sub-personality) working in principally the same "constructive" (inductive and—by the means of "transfer"-mechanisms—also deductive) way as the critical instances of verbalized measurement and computation do. The difference seems to be merely one of degree. As also has been shown by experiment, the perceptual system is—as compared, in a functional sense, with discoursive methods of knowledge—relatively inertial, stereotyped, superficial, confused, unanalytical, and sometimes narrow in its admission of and its ways of evaluating cues. The biologically required quickness of functioning and the mechanization involved make the contents of perception look more immediate in their appearance than the final outcomes of explicit operations do. In the same way as there is a primitive layer of our action system—the instinctlike "Id" of psychoanalysis—, the perceptual system is representing the primitive and in some sense blind, or "stupid," layer of our cognitive system. Perception, like the Id, does not "know" explicitly about its (stimulus-) motives and about the load of past experience functionally involved in each of its acts. There is no "sensual" layer or type of datum except functionally speaking. The functionally

direct (e.g., the retinal size) may seem phenomenally most unnatural to realize, whereas the phenomenally simplest may be functionally most complex.

It is a merit of the totalitarian ("molar" instead of "molecular") behavioristic school represented by Tolman that purposive consciousness-terms like "expectation" or "hypothesis" (Krechevsky) have been redefined in objective, analytic terms. As was pointed out above, in this way of description of perception vs. physical measurement-approach, both appear to be fundamentally equal in their way of inductive functioning and not more than gradually different with respect to the correctness or "dullness" of their hypotheses, or the reliability and cleanness of intentional attainment of objects.

The possibility may not be excluded of attaining perceptually through sufficient training even types of objects which are mediated very indirectly, that is by very long and complex chains of causes and inference. A highly mechanized physicist may one day reach a point which in some sense could be considered to be a phenomenally immediate perception of remotely inferred constructs like an atom, or of the fulfillment of any conceptual requirement by a certain fact (as, e.g, of constancy of acceleration). This is to emphasize again, in a drastic way, the functional uselessness of the concept of the immediately given. With sufficient mechanization and in proper attitude, any abstract construct may become as accessible and "*anschaulich*" as any of our natural intuitive perceptions. From the standpoint of an instantaneous phenomenal analysis, constructs like atoms, then, would become as indubitable, or as dubitable, as the most "sensual" perception could be. The mediating events, formerly explicitly given, would become lost in their new rôle of mere mediation. And, in most cases, perception would also become able to shift arbitrarily between the two (or more) established ways of focalization, the old and the new. This lability, too, makes it clear that no epistemological (metaphysical) distinction could be successfully based upon the vague and variable phenomenological (introspective) distinction between a (supposedly indubitable) immediate given and a (supposedly dubitable) derived construct.

As a last problem we may touch in short the problem of partition (*Schnitt*) between observer (subject) and observed object as it has been raised in recent publications in physics.[32] According to the current view, the boundary between the observer and his object can be arbitrarily displaced.

Considering first a single perceptual act, we would state that no freedom is given at all as to the interpretation of the partition between subject and object, since this layer will be univocally dictated by the findings of constancy-research upon this act. In the case of a complete thing-constancy the partition is placed at the body far away, and the light rays have to be considered to belong to the observer. The same would hold also in the case of a stick touching and correctly mediating the surface qualities of a distant body, whereas at the same time the hand holding the "probe" is completely extinguished in its phenomenal representation. In the opposite case of a complete projective stimulus-constancy, the light rays arriving at the retina or the stick would become the object observed. And in the usual case of an incomplete compromise-solution, the partition would lie—conceptually, but not spatially—in-between the proximal stimulus and the distant body.

Regarding secondly the perceptual system as a whole, however, some arbitrariness in the layer of partition is given by the fact that, by a shift in attitude, the type of object of perception can be changed. But, as it has been pointed out above, this shift-span has its certain narrowly circumscribed limits; so, e.g., no complete shift may be expected along the scale between size-constancy and retinal comparison.

These two criticisms do not touch the correct statement in the theory of partition that a physicist with his set-up of scientific tools and his abstract methods of measurement and computation has a much higher degree of freedom in focalizing and shifting partition than has the more habitual, stereotyped and inertial system of intuitive perception. But even in this case a subsequent description would be predetermined as to the "object" of

[32] See the discussion of the problem by V. F. Lenzen, "The interaction of subject and object in observation," Second internat. congress for the unity of science, Kopenhagen 1936.

the procedure by the nature and success of the technical and conceptual operations involved.

In conclusion: the primary subject-matter of psychology is defined by a formal criterion as the objective pattern of couplings which an organism, in its causal intercourse with the environment, was able to focalize in a fairly "constant" way upon more or less remote (life-sustaining) types of "objects," despite the disturbing variability (multiplicity and ambiguity) of the single mediating stimulus-cues and means. Psychology is, therefore, a science of the relational achievements at the command of the organism, of well established far-reaching (cognitive or effective) success—quantifiable in terms of its "objects attained"—, rather than of mediation processes, as such. (In short, psychology is a science in terms of "what" rather than of "how.") Troublesome older "philosophical" problems, or pseudo-problems,—like dualism, meaning, intentionality, subjectivity, totality, immediate given—appear, within this system of a "psychology in terms of objects," in a revised, objectified form; they develop into problems of analytical psychological experimentation and conceptual reduction by an operational shift in their definition, thus disclosing the soundness of feeling underlying the original insufficient statement of these complex problems.

Arranging the Sciences: I. An Experiment

BY

WILLIAM MARIAS MALISOFF

ONE of the most thankless tasks in the field of the philosophy of science is the arranging of the sciences. If philosophy is to fulfill the partial rôle of being the "science of the sciences" it is sure to return to this undertaking time and time again. Sometimes it will refer to the enterprise as "the classification of the sciences," sometimes as the formulation of "the system of the sciences," and occasionally in the grand manner as a description of the "order of Nature." I prefer to speak of "arranging" the sciences.

The reasons for the preference rest on very permissible doubts. I doubt that the sciences constitute a class. If they are not a class, how can one speak of classification? I next attend to the impulse to make a class out of them. I find that I have to do more than a little violence to make them fit, and that if I cruelly lop off a good deal in the various sciences which is only traditional or decadent (in my opinion) I am left without the old names and with only a dying hope of identifying the survivors of the operation to the satisfaction of the obstinate tribes of scientists. Yet even such a romantic enterprise would be worth a try if it could really set the house in order. Unfortunately, the history of such forced "classifications" clearly shows the smallness of the chance of discovering now or soon *the* one and only or the optimum classification.[1]

[1] No more than this emerges from even so laborious an analysis as given by Carl Stumpf, "Zur Einteilung der Wissenschaften," K. Akademie der Wissenschaften, Berlin, 1906, V, 1–93.

I doubt that the sciences constitute a system. The term implies a degree of coherence and synthesis which the subject simply does not possess. It is used by those who believe that some of the sciences at least form natural classes, that they hang together naturally by their "object-matter." Believe it or not, but how can one prove it?[2]

Most of all do I doubt that the sciences can reveal through any mere grouping "the order of Nature." Even if the expression were clearer there would still be the danger of shutting our eyes to the perennial incompleteness and conventionality of empirical science. The placing of "truth" at the end of an infinite series of investigations only helps to fertilize the creativeness of scientists, whereas it frightens theologians. There is no necessity for an immediate revelation. That would be quite a calamity to a scientist.

Setting aside the temptation to enter into a historical account and analysis of proposals from the time of the Egyptians till today in order to make an "arrangement of the arrangements," I wish to present by example an experiment in arrangement in which the guiding motives are: (1) to do as little violence as possible to the sciences as they stand, (2) to pick a flexible but not too complex a form allowing for the reception of newcomers at almost any point, (3) to allow for a maximum of crossings, in a simple form, like a plane, tending to *break down* classifications, and last, but far from least, (4) to suggest a few newcomers. Of the principles used, greatest success has been registered by net works or tables, or circular and spiral arrangements. I choose for this experiment the circular, because it is after all a table with ends joined permitting of greater crossings. This is the general character of an Encyclopaedia. The limitation to two dimensions is regrettable, but greater complexity seems to be quite unmanageable.

Figure 1 shows in the core 3 groups of sciences: the abstract, the "natural," the human. They are one in some respect, three

[2] See the interesting article of H. E. Bliss, *Philosophy of Science* Vol. 2, 86–103 (1935), who is interested in library organization. That, however, must take care of many minutiae, authors' errors and painstaking cross-indexing. Discovery can actually be hampered by such "accidents."

only by virtue of a historic but not a necessary differentiation. The next circle is the residence for hybrid groups which are not named, specific names being required rather in hybrid individuals.

The next layer splits each group into two, leaving two sectors, one as a neighbor to the psychological sciences, another as a

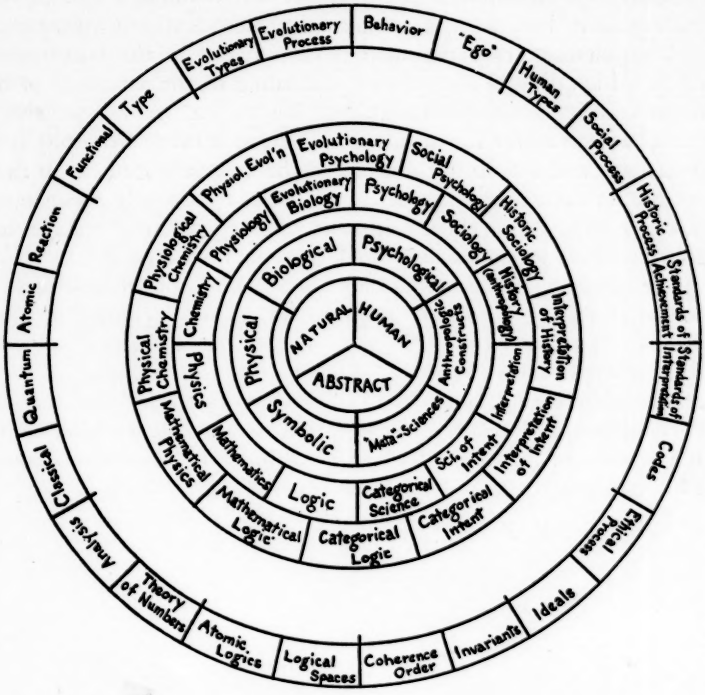

Fig. 1. An Arrangement of the Sciences

neighbor to the sciences dealing with symbols. Those are just a hope. The next circle again is introduced as a reminder of possible overlappings.

Further division into groups of two produces the usual sequence of the sciences, embodying some of the advantages of Comte's

proposals, bent into an arc and allowing for a closure by four new sciences, given the tentative names: history (including anthropology), interpretation, science of intent, science of categories. In the very next circle the overlapping sciences are named, of which many are already quite developed. It takes a little courage to admit that the choice was made in the hope of finding room here for the *sciences* of ethics, aesthetics, religion.

A supplementary experiment is carried out in the outermost circle, which divides each science according to the attention paid to continuity and discontinuity. Tentative, very tentative, names are given for sub-sciences. The cue is taken from physics (relativity and quantum theory) and the atomic is set next to the atomic for each succeeding science. This is done only to suggest new regroupings for those scientists who are powerful enough to effect reforms in their sciences (if they prove the better).

For those who are wondering about the "applied sciences," suffice it to say here that there is no essential change in the attack. There is a change in names, reflecting the return to facts from theory called practice. One might, for example, pick a trio, as engineering, eugenics and "the life of reason," and with a very liberal and broad interpretation arrange everything from the science of making calculating machines to Marxism. Soon I hope to present this as a second experiment.

New York, N. Y.

Unified Science and Its Encyclopaedia

BY

OTTO NEURATH

I

The movement for the Unity of Science has been making headway for a number of years, and each year more workers in the various scientific domains have expressed their interest in it by participating in the discussion of its aims and its specific problems. A characteristic feature of this movement which explains this growing interest is that it does not propose a "super-science" which is to legislate to the special sciences. Its proposal of a Unity of Science is of a different kind, not based upon some grand metaphysical view of any sort. Those who are active within this movement are emphatic in their insistence that instead of aiming at a synthesis of the different sciences on the basis of a prior and independent philosophy, the special sciences will themselves supply their own synthesizing glue. For appreciating the significance of this movement it is therefore necessary to keep in mind that its tendency is toward a *unified science*, departmentalized into special sciences, and not toward an artificial and speculative juxtaposition of an autonomous philosophy *and* an autonomous group of sciences.

The key to this movement is to be found in the fact that the special sciences themselves exhibit in various ways the need for such a unification. For example, the different psychological

theories employ so many different terms and phrases that it becomes difficult to know whether they are dealing with the same subject or not. Thus, individual psychologists analyze the case of a man who becomes ill when he finds an examination more difficult than he had expected; reflexologists discuss the case of a dog which becomes ill when it is required to discriminate between sound signals that are too complicated for it, as a condition to obtain food. And yet these different psychological schools employ completely different terms and rules for combining them when they explain these essentially similar situations. Gestalt psychologists use a distinctive formulation quite different from other schools of psychology, when they explain, for example, the deliberative activity of monkeys seeking food; and behaviorists, in accounting for the trial and error activity of rats seeking food use a still different language. The difficulty of communication is further increased when we examine the formulations of psychoanalysts. It is natural to seek a common platform for all these different investigations and theories. Similar illustrations could be given from biology, sociology and other sciences.

Many difficulties occur in connection with problems which are on the borderline between the different sciences, for example, at the points of contact between biology and physics. This is especially the case when the larger issues between vitalism and its opponents are being discussed. In a great many cases it is not clear whether the difficulties arise because of a defective formulation of the problems or because of defective and insufficient investigations.

One of the most important aims of the Unity of Science movement is therefore concerned with the unification of scientific language. Distinct terms occur in different disciplines which nevertheless may have the same function; and much fruitless controversy may arise in trying to find a distinction between them. It is, for instance, an open question whether in biology the special terms "stimulus" and "reaction" are needed, or whether the usual physical terms "cause" and "effect" are sufficient. A large collection of terms have been gathered by the various

sciences during the centuries, and it is necessary to examine this collection from time to time, for terms should not be multiplied beyond necessity.

Such a movement toward a unified scientific language is fundamental for eliminating a great many so-called metaphysical questions. For example, within the last few years heated discussions have occurred over the problem of the "freedom of the will" in connection with the theory that an atom is "free" to choose different paths for its motion. Whether there is an intimate connection between the behavior of atoms and of living organisms is of course a genuine problem, but it has no relevance to the one mentioned above.

It is by no means an easy task to unify scientific language. A great many terms of the successful special sciences are not univocal and clear. This is especially the case when new theories are in the process of growth. It is not certain in such cases that the new terms and formulations will continue to be employed in the future. Our attempt to systematize the science must leave those questions which can be decided only by future investigations. And we must keep in mind that future investigations will themselves evolve new open questions. It is therefore misleading as well as confusing to speak in anticipation about THE SYSTEM of science, as if the form and content of such a system were determined once for all. The unification and systematization of science are permanent activities.

It will be clear that the attitude here indicated is incompatible with one which assumes certain fixed elements, which are forever beyond dispute. Those philosophers who operate with such fixed elements, *a priori* principles, absolute truths or similar apparatus are simply continuing the Cartesian tradition. Descartes supposed all our daily knowledge and experience eliminated in order that he might start anew with a handful of indubitable truths upon which to erect all the sciences. In a certain sense that is also the tradition of Kantianism and similar schools, even though further qualifications would have to be added to differentiate these schools from one another.

The Unity of Science movement is part and parcel of the broad

stream of scientific empiricism which is an extension and development of our daily experience, employing initially the language of every-day use. In the main this is an attitude similar to the Commonsensism of Peirce, the "natural world view" of Avenarius and the theory of experience of Dewey. According to this outlook, all the predictions of the sciences start with and are controlled by observation-statements formulated in our every-day language. We begin with terms at first very vague, but which can immediately be seen to be capable of greater precision. The more one reflects upon this matter the more one is lead to abandon the notion of *absolutely precise terms* even as limits of a refining process. The supposition that there are such limits seems to me to be another illustration of what may be called "Pseudo-Rationalism."

II

The program of the unification of scientific language requires a logical analysis of sciences. The history of science during the last decades shows the importance of such an analysis for the progress of concrete scientific work. Within the framework of the general critique of the traditional concepts of space and time the analysis of Ernst Mach was of fundamental importance in preparing the way for Einstein's theory of relativity. Perhaps a very remarkable feature of Mach's work is the fact that his far reaching conclusions did not depend upon new experimental data, but simply upon a rigorous logical analysis of the traditional formulations. He of course employed all the knowledge common to physicists of his day. Mach's work is a milestone in the evolution of the modern analysis of sciences which was further developed by Poincaré, Duhem, Boltzmann, Bertrand Russell and others.

The technique of such logical analysis was gradually perfected and employed consciously. As a consequence, feats of logical analysis which were heretofore possible only to men of genius may now be taught systematically to scientists of ordinary attainments. This method of analysis is the subject matter of the new discipline called the *Logic of Science.*

What is called modern symbolic logic or logistic has been cultivated not only as an autonomous discipline but always in more or less close reference to its use as an instrument of the logical analysis of the sciences. The evolution of modern logic makes possible the organization and utilization of all research as upon the foundations of mathematics; and at the same time it clarifies the application of mathematical and other calculi to concrete subject matter. We have thus become clearer concerning such matters as the relation between what are called pure and physical geometry, or the logical structure of the probability calculus and its relevance to all the concrete sciences.

The contacts between modern logistic and the modern analysis of science have already begun to alter the general scientific attitude of our period. Even those workers who have not even an inkling of the logistic calculus or who make no use of it as an instrument, nevertheless can carry on scientific research in the spirit of modern logical empiricism.

In the history of human thought logic was frequently cultivated by metaphysically oriented philosophers, especially by different types of rationalists. As a consequence, scientific empiricism received the support of workers using different methods of empirical research and of applied mathematicians—but not of logicians. But as an outcome of the fact that modern logic has as one of its rôles the task of supporting the empirical sciences, the empiricism of our time has acquired an altered physiognomy. The logical calculus in its widest sense becomes an essential apparatus of a unified science, and all logistic research acquires very great direct and indirect importance for the evolution of the logic of science.

The task of modern logical analysis of science is to build up a more consistent framework for the special science and for a unified science. The process of the logical organization of a single science cannot be divorced from the process of building up bridges or connections between the different sciences. A logical empiricist in carrying through an analysis within one science must always keep his eye on the interrelations of all the different disciplines. A concern of this sort for the logical or-

ganization of the sciences, which aims at a comprehensive and suggestive survey of them, is an alternative to and a possible substitute for the traditional philosophical attitude.

As has already been indicated, the unification of scientific language is a special and technical task. The fundamental thesis of our movement is that terms similar to those employed in physics and in our every-day language are sufficient for constructing all sciences. This thesis, known as Physicalism, has been progressively confirmed by special investigations in recent years.

Within the broad framework of Physicalism a great many different proposals may be discussed. Various attempts to formulate the program and technique of Physicalism have been made within the Viennese circle, and within other groups having a similar orientation. Of greatest importance are those techniques which show us how to use consistently the language of daily life as the basis for all our scientific work, and to correlate the more or less complicated formulas of science with the statements in our every-day language. Carnap has proposed certain special devices which make possible the introduction of terms so that they become dependent upon physical terms without, however, being "defined" by the latter in the strict sense of the word. A very important discussion is now in progress concerning the manner in which all scientific terms depend upon physical statements and the manner in which all scientific laws depend upon physical laws. For instance, one problem is whether biological statements can be *formulated* by means of physical terms, exclusively; a problem distinct from this one is whether biological laws can be *derived* from physical laws. It is possible that at a given time, for example, the present, the former could be affirmed while the latter is denied. This nest of problems is of particular importance in the fields of biology, the theory of behavior and sociology.

A further problem of great importance is the manner in which we can incorporate into one language statements occurring in educational discussions, discussions about art, ethics, law and other subjects like these. It is possible that the result of such

attempts to incorporate statements of different kinds within the framework of the language of scientific empiricism will lead to a change of the traditional demarcations between the different separate disciplines. Some interesting possibilities are already apparent. The fruitfulness of the unity of science movement is indicated by the fact that more and more students of special fields participate in the discussion of the relation of their discipline to other domains. In this way bridges between different fields are constructed at points where only isolated domains were found before.

III

If we reject the rationalistic anticipation of *the* system of the sciences, if we reject the notion of a philosophical system which is to legislate for the sciences, what is the maximum coördination of the sciences which remains possible? The only answer that can be given for the time being is: *An Encyclopaedia of the Sciences.*

It is toward the realization of such an encyclopaedia that the present efforts of the International Unity of Science Movement are directed. The initial plans for such an encyclopaedia were proposed many years ago within the Viennese Circle. The first public formulation of this plan was presented to the First International Congress for the Unity of Science in Paris, 1935[1] which agreed to collaborate in the project and to promote it. The Second Congress, held in Copenhagen, in 1936, resolved that the theme of the Third Congress, which is to be held in Paris, July 29 to 31, 1937, is to be the encyclopaedia.

A committee consisting of Rudolf Carnap (Chicago), Philipp Frank (Prague), Joergen Joergensen (Copenhagen), Charles W. Morris (Chicago), Otto Neurath (The Hague), Louis Rougier

[1] See Otto Neurath, "Une Encyclopedie Internationale de la Science Unitaire.", in *Actes du Congres Internationale de Philosophie* Scientifique, Part II. Hermann & Cie., Paris, 1936. Charles W. Morris, "Remarks on the Proposed Encylopaedia" Ibid; Philipp Frank, "Diskussionsbemerkung zur Encyclopaedie, "Ibid; Rudolf Carnap, "Ueber die Einheitsprache der Wissenschaft. Logische Bermerkungen zum Projekt einer Encyclopaedie," Ibid. A brief appreciation by Bertrand Russell of the aims of the Encyclopaedia will also be found in the first part of the *Actes*, in the paper entitled, "The Congress of Scientific Philosophy."

(Besançon and Cairo), is at work upon a detailed prospectus of the encyclopaedia. The University of Chicago Press has already arranged to publish the first two volumes, which are to appear in 1939, in time for the Fifth International Congress for the Unity of Science, to be held at Harvard University. This arrangement is contingent upon there being a sufficiently large number of subscribers. The estimated subscription price of one volume is about $7.50, while the price for non-subscribers will be about $10.00.

The new encyclopaedia is so planned that it will provide an opportunity for a meeting of minds of scientific workers of different kinds. For the project is based on the idea of scientific tolerance within the framework of scientific empiricism. This will be clear from some details concerning the early volumes of the work.

The plan is to publish two volumes at first, containing sixteen to twenty pamphlets on the Foundations of the Sciences. These will therefore contain articles on the Logical Analysis of Science, General Linguistics, Mathematics and Logic, Scientific Procedures of Empirical Sciences, Probability, Physics, Cosmology, Biology, Theory of Behavior, Social Sciences, Empirical Axiology, History of Science, History of Logic, History of Empiricism, Logical Empiricism, as well as a bibliographical survey and an index to the volumes. The following have already agreed to collaborate in the writing of these volumes: *Andrade, Brunswik, Carnap, Dewey, Frank, Joergensen, Lenzen, Mainx, Morris, Nagel, Neurath;* and a number of other scholars have also been approached for contributions to these as well as subsequent volumes.

In later volumes of the Encyclopaedia the attempt will be made to show the differences in conclusions reached and points of view maintained in the special sciences, and it is planned to give people with differing opinions an opportunity to explain their standpoints. It will be a special aim of this Encyclopaedia to show the gaps in our present knowledge and the difficulties which are found at present in various fields in which new ideas are growing. Such an encyclopaedia which makes provisions for showing gaps and conflicting points of view, and which emphasizes the

incompleteness of our knowledge, is designed especially for people in the process of growth and development.

It is also planned that the collaborators are to remain in permanent contact with one another, so that the encyclopaedia may serve the building of bridges from one science to another, without, however, limiting personal expressions of opinion. Without pursuing utopian ideals, the effort will be made to have the scientific language of the encyclopaedia as homogeneous as it is possible to make it at present. It is planned, consequently, to develop a unified vocabulary of the main concepts of a scientific empiricism. For the selection of a suitable terminology the collaborators will consider the translations of different terms used in English, French and German. Such a nucleus of a trilingual dictionary of scientific empiricism will undoubtedly possess much educational value.

The Encyclopaedia will appear in English at first, to be followed, if it is found feasible, by editions in other languages.

Reports will be published from time to time concerning the progress made in realizing the plans for the Encyclopaedia, as well as any important discussions concerning the "bridges" constructed between the various sciences, the unification of scientific language, and the elimination of superflous or dangerous terms. The editors of THE PHILOSOPHY OF SCIENCE are prepared to provide a special page for the "new encyclopaedists."

IV

It may be of interest to the reader if I say something about the origin of the idea of this encyclopaedia. My own intellectual development in the direction of a comprehensive scientific view was influenced by Mach, Poincaré, and other modern thinkers, and especially by Gregorius Itelson. My central conviction became that the elaboration of the differences between the various sciences is an unessential task, but that, on the contrary, it was especially important to develop an account of all the sciences using only one kind of a scientific "style." That is to say, I became convinced of the possibility of speaking about the stars and about men with the same logical techniques and

with the same scientific dispassionateness. I also realized that such a comprehensive scientific view could serve as an important basis for a sound general education. In this way there occurred to some friends and myself the idea of working up a series of about 100 pamphlets which would deal with all sorts of things,—stars, stones, plants, animals and men. A comprehensive alphabetical index was to be included, so that this collection could be used as a dictionary as well. This educational project was heartily welcomed by many, including Philipp Frank and Einstein. The latter wrote me that such a well planned collection would serve the same function for the masses today as did the French Encyclopaedia for the intellectual groups in France during the 18th century. However, it was essential for this project that all the texts be accompanied by illustrations. This plan could not be worked out, and our various discussions and attempts to begin the series were without success.

Two stimuli influenced me in the following years. As director of a museum in Vienna founded on Visual Education, I, together with my collaborators Gerd Arntz, Friedrich Bauermeister, Erwin Bernath, Marie Reidemeister and others, became profoundly interested in developing a consistent method of visual education. Contacts with a great many countries stimulated this activity, and we began an International Visual Language with a Visual Dictionary and a Visual Grammar. Our object was to build up a visual thesaurus, a store of pictographs and other pictorial representations, with an accompanying explanatory text. We found it possible to publish only one large atlas containing 100 maps and pictographs with statistics and text: "Gesellschaft und Wirtschaft," published by the Bibliographisches Institut, Leipzig; but this was intended simply as the first item in a whole series of publications. This work was similar in certain respects to the attempts of Paul Otlet, Director of the Palais Mondial (Bruxelles), who promoted the "Mundaneum" idea of preparing exhibitions and publishing books with illustrations all within a comprehensive framework. Our group consequently planned to coöperate with Otlet. But it was not possible to accomplish any concrete work, and the idea of a new Orbis

Pictus, of Visual Thesaurus remained, and still remains an idea, a plan for the future.

During the same period there developed in Vienna what is known as the Viennese Circle. Moritz Schlick, Professor at the University of Vienna, was a stimulating influence upon a group of men interested in the logical problems of science, and he was one of the first thinkers who recognized and explained the importance of Einstein's Theory of Relativity for modern Empiricism. In the interest of advancing scientific empiricism he, together with Hans Hahn, was instrumental in having Rudolf Carnap come to Vienna, and it was Carnap's special contribution to develop modern logic as an essential tool for scientific analysis. Schlick, together with Philipp Frank (Prague), one of the leading figures in the Viennese Circle, became the editor of the series "Schriften zur Wissenschaftlichen Weltauffassung." Bertrand Russell also became an important influence upon members of this group, especially upon all those interested in mathematics and logistic. Wittgenstein's philosophy was carefully read and discussed, and the Viennese Circle concurred in his view, that we do not have special philosophical statements. Philosophy was regarded by a great many members of the Viennese Circle as the analysis of science and the discussion of its logical problems, not as a special superscience.

However, within this discussion group, I objected to Wittgenstein's metaphysical tendencies and proposed to take the consequences of the thesis, that we have only scientific statements. In other words, I maintained that we must build up the sciences as a body of empirical statements, and discuss all problems concerning them within the scientific framework. I proposed to call this complex of statements: Unified Science. I thought it would be useful to show that we could assemble the different discussions of scientific problems into a unified collection, and together with Carnap, Hahn, Joergensen, and now Morris, I began to edit the series "Einheitswissenschaft," with the aim of breaking ground for a more systematic unification of the sciences.

The trend of my arguments led me to emphasize the importance of a purified every day language ("universal slang") as the lan-

guage of physicalism. By rejecting the various attempts to construct a special "world of things" and a special "world of thinking" juxtaposed in opposition to one another, we are in the position to unify our scientific language, eliminate the use of two languages "physical language" and "phenomenal language," and to advance the work of instituting the common language of physicalism.

If we use the universal slang as the basis for all our predictions and for their control, we discover that the statements we formulate can always be corrected when we concentrate our attention upon special points. This suggests that it is misleading to use such terms as "verification" and "refutation," because of their absolutistic flavor; and I therefore proposed that we employ the terms "confirmation" and "disconfirmation." This is not the place to discuss the logical consequences of this proposal. In brief, however, its adoption destroys the last vestiges of a philosophical absolutism, whether it be the absolutism of exactness, of "the" truth, or similar dogmas. We possess no fixed point which may be made the fulcrum for moving the earth; and in like manner we have no absolutely firm ground upon which to establish the sciences. Our actual situation is as if we were on board ship on an open sea and were required to change various parts of the ship during the voyage. We cannot find an absolute immutable basis for science; and our various discussions can only determine whether scientific statements are accepted by a more or less determinate number of scientists and other men. New ideas may be compared with those *historically* accepted by the sciences, but not with an unalterable standard of truth. I will add that views similar to these were developed by Carnap, Frank, Morris and others.

This approach is fundamentally opposed to every conception which employs the notion of *the system* as the "limit" of scientific research. And it seems natural and an immediate consequence of these views that the idea of a "unified science" be realized in an Encyclopaedia as a "model" of our knowledge. For since we cannot compare the historically given sciences with "the real science", the most we can achieve in our scientific work seems to

to be an Encyclopaedia, constructed coöperatively by scientists who are interested in scientific empiricism. This program may be called "Encyclopaedism."

That is the general background for my proposal of an international encyclopaedia to embody the idea of a unified science. Such an encyclopaedia will show that scientists, though working in different fields and different countries, may nevertheless coöperate as successfully within this wide field as when they normally coöperate within such special fields as physics or mathematics. Such an encyclopaedia will exhibit the logical framework of logical empiricism, and will be a mainstay of scientific empiricism in general as well as of the unity of science movement in the widest sense.

Considerable interest in the Encyclopaedia project has already been shown by men of science and by the educated public. It is hoped that a larger public will be won for the Encyclopaedia in all parts of the world, and that suggestions from different sources will stimulate the activity of the collaborators and organizers of the Encyclopaedia. In this way the Encyclopaedia will become a living intellectual force growing out of a living need, and not a mausoleum or a herbarium.

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DISCUSSION (CORRESPONDENCE)

In this section we shall publish brief communications commenting on published papers or bringing up special points of interest which can be discussed by correspondence.

ON LOGICS THAT ARE MORE OR LESS ABSTRACT

Sir:

Much ink has been spilt in discussing the question by what baptismal name the new logic shall be known. Mathematical or symbolic logic designates no more than the extension and refinement of a method already employed by Aristotle in the Organon. Why is it that logic can not lose all its adjectives? Why is it that we can not speak of logic as such?

Historically, of course, the explanation is that, with the rapid advance of formal or mathematical methods in the middle of the last century, there were already competing logics in the field, logics that could neither absorb nor recognize the new method because they had already developed methods of their own, the logic of empiricism, which descends to us from Bacon and Mill, the logic of idealism, which Hegel himself, I think, described as a warmed up dish for which he himself had provided the sauce. And then there is that vast army of honest folk who detest mathematics and all of her works, who reject any instrument, delicate and powerful though it may be, much as the workman of today prefers his old tools to the refinements of labor saving machines. These misologists, these "haters of the reason", had to be taken account of too, and so, lest there should be a misunderstanding, it seemed better to be perfectly frank. We are dealing, these pioneers in the field were accustomed to say, with mathematical or symbolic logic. Boole even called his work "The Laws of Thought", but this raised a difficulty, which Mr. Russell pointed out later on, of why no one had ever thought of thinking that way before. Kant and others have thought of the same difficulty.

In one of the numbers of this journal some time ago this writer published an article under the title of Abstract Logic and he has been asked since then by a number of persons just what this new adjective abstract was intended to mean. The writer wishes that he had that fine talent of the Italian school for inventing a name that will "stick", and while this term is not likely to stick, he would like to explain its intent.

When Mr. Lewis attached to his logic the adjective "strict" he was seeking a principium individuationis. He was interested in separating himself from the goatherds. While the present writer's modal logic is in disagreement in many important respects with Mr. Lewis' strict implication, his invention of the term abstract, as designating modal logic, is motivated in much the same way. This writer, always the victim of etymologies, has never been able to avoid the tacit dichotomy of what is strict and what is loose. Very well, the term abstract was intended by this writer to indicate very explicitly the difference between the logician who recognizes the distinction between the actual and the possible, and the logician who refuses to recognize any such distinction.

In order to make the far reaching character of this divergence of view point perfectly clear to the lay mind let me recount a bit of personal history. Some time ago this writer sent an article on modal logic to a journal largely controlled by a group of young gentlemen who move about in a framework of ideas that is more loose than strict, (in the sense above defined). It was combed over for mistakes by the referee and at the end of six months was returned with this comment: (1) not one word of it was intelligible and (2) it contained a contradiction.

The first comment I could understand, because the paper was concerned with making perfectly clear and unambiguous the meaning of the necessary, the actual and the possible, and in drawing from these meanings their ultimate mathematical consequences, whereas the referee has seemed to make it clear from his past writings that he does not recognize any of these modal distinctions at all.

As to the second objection, that the paper contained a contradiction —and here I invite the lay mind to concentrate—the case was this: I had said of certain expansion formulas, that they *failed* within a certain region. The referee found a case in which they did not fail in that region and then said that I had contradicted myself. Now as a perfectly good modalist in the Aristotelian tradition, I meant by "fail

within a certain region", that one could find special values of the variables within that region for which the formulas do not hold. But for my critic, thinking within the framework of the Russell-Hilbert algebra of propositions, to fail meant to fail in all cases. The editor, while he said the criticism was fallacious, missed the point of the fallacy himself. He doesn't believe either, as he assured me privately, that modal distinctions have any meaning.

The writer hopes that the reader will not accuse him of harboring any personal pique concerning the fate of a paper that has since appeared elsewhere. His concern is rather for the fate of other contributors to a journal whose editorial policy is too largely controlled by a part of a board which can only speak and think "in one tongue". Or, to repeat a phrase invented by the liberal editor of this journal, controlled by "minds that do not transplant".

Lately this author has been gathering together the scattered papers he has published on modality in order to give the whole subject a more ordered and systematic treatment. In order to give again and more objectively some idea of this controversy between the modalists and the anti-modalists, he would like to quote a few paragraphs from the preface of this work which he has planned.

In evaluating the claims of any formulation of logical theory, there is not only the question, "Are the postulates of the system consistent?" There is besides this the question, "Do they lead to results that are completely general?" Thus, if an axiom or theorem should somewhere appear, that wipes out some essential distinction, then the generality of our science would be reduced. If we should, for example, equate

$$p < q = p \text{ implies } q$$

to the disjunction,

$$p' + q = p(\text{is untrue}) \text{ or } q(\text{is true})$$

we should destroy the essential difference between p is untrue and p is impossible, we should not distinguish between "not necessarily true" and "necessarily not true". If p fails in one instance, it is untrue, but to be impossible it must fail in all instances.

Let no one in the simplicity of his heart imagine that this is an academic and not a practical issue, for suppose that the solution of some paradox, like the paradox of Epimenides, should depend on our making the proper modal distinctions. Then a logic which fails to do this must, with whatever burden of disappointment, declare such statements to be meaningless. Nor will any patching up in the sequel remedy the

loss of an adequate instrument. It will not help matters to say: a proposition can not apply to itself, for suppose I assert:

"Every proposition is either true or false".

Then this must be amended to read:

> "Every proposition is either true or false, except what I have just said, and that is meaningless."

Will any candid soul deny that the cure is worse than the disease? No, our science must be one that sets no limitations on the meaning of its variables.

The so called theory of types, as we know it today, is only necessary because the logic which goes with it is inadequate. But the theory of types is far from being something new in the world, as anyone familiar with his Prantl (*Geschichte der Logik im Abendlande*) is well aware. A paradox somewhat more general than that of Epimenides is stated by Albertus de Saxonia, writing in Vienna before 1390:

> Ponatur quod Socrates dicat illam, "Plato dicit falsum" et Plato dicat illam, "Socrates dicit verum."

Peter von Ailly distinguishes between written and mental propositions and says:

> Nulla propositio mentalis proprie dicta potest significare se ipsam esse falsam,

and in general the dictum of correct method becomes:

Pars propositionis non potest supponere pro toto.

Grelling-Nelson's criticism of Russell might easily have been lifted out of Wycliffe's *Tractatus de Logica* had he known it was there. The great service of *Principia Mathematica* was that its authors brought these paradoxes to the attention of the scientific world in a manner that could not be ignored.

Now that the issue between those that believe that modal distinctions are essential and those that do not is becoming more and more clearly drawn, might it not be useful to employ a term like abstract logic to designate the logic of modality? Those who are working in this field have a kind of common faith, however much they may differ among themselves in matters of detail.

Henry Bradford Smith.

University of Pennsylvania.

THE MEANING OF MEANING

Sir:

I am unable to see that Mr. Biederman's criticism affects my position in any significant way. It is true that a tautology is any proposition which "cannot conceivably be false." But this phrase demands clarification. The truth or falsity of a proposition is a matter of its relations either to facts (extension) or to symbols (intension). To say that a proposition is inconceivably false (necessarily true), or conceivably false (possibly false), I take it, implies a recognition only of certain of its intensional relations. Thus the statement, "P is inconceivably false," means "There exists a necessarily true postulate scheme which implies P;" and the statement, "P is conceivably false," means "There exists no necessarily true postulate scheme which implies P." Newton's law is therefore a tautology relative to the postulate scheme from which it can be deduced, and within this scheme it cannot conceivably be false, i.e. it exhausts the possibilities permitted by the system. Similarly, a law of logic is a tautology relative to a certain group of postulates, and within this system it cannot conceivably be false. Thus any proposition whatsoever is conceivably false until the proper postulate system is found. After such a system is found the proposition is a tautology, exhaustive of the possibilities, necessarily true, and inconceivably false—all with reference to the postulate scheme in question.

All of this is true on the grounds of any theory of meaning. The empirical theory of meaning insists upon the impossibility of considering necessary truth in any final sense apart from extensional truth. A proposition may be extensionally true but conceivably false. An extensionally true proposition becomes inconceivably false when the necessarily true postulate system has been found. But such a postulate system is itself necessarily true because it implies a large range of propositions which are extensionally true and which constitute its verification. This means, in effect, that a proposition which is extensionally true is also necessarily true (inconceivably false, exhaustive of possibilities, tautological) just to the extent to which a larger, rather than a smaller, class of propositions can be found all of which (including the given proposition) are extensionally true and deducible from a common postulate system. An isolated proposition is always conceivably false. Such a proposition acquires necessary truth if it can be shown to be compatible with a large group of extensionally true propositions.

A. Cornelius Benjamin.

A NEW CLASSIFICATION OF STATISTICS

Sir:

This note contains a criticism of the way statistics has been handled; and tries to prove that some of the trouble can be eliminated by means of a new classification of statistics.

The following survey of the field of statistics is presupposed by the remainder of this note: The modern field of statistics can be divided into two main parts, statistical method and applied statistics. Statistical method attempts to formulate rules of procedure applicable in handling groups of facts of many different varieties. This method may be applied by a sociologist, an economist, an insurance actuary, a psychologist, a biologist, a physicist, an astronomer, or an investigator in almost any other branch of human knowledge.

Statistical method has been sometimes considered a branch of mathematics. Is this proper? Mathematical statistical method should be regarded as a doctrine composed of two sets of propositions: an assumed set, called a system of postulates; and a set logically deducible from the postulates, and called theorems. These theorems ought to be applicable to observed facts. When mathematical statistical method is applied, data are frequently warped to fit statistical convenience. Although it is fictitious accuracy to be too finical about warping, wrenching the facts sometimes causes important errors. If statistical method is to fit the facts with sufficient accuracy, a place must be left open for an inductive procedure which starts with the observed facts and endeavors so to group them and treat them that propositions regarding the facts are made apparent. Interpreting is one of the special statistical methods. It is frequently impossible to interpret satisfactorily the statistical facts by the use of mere mathematical methods. For example, it is a matter of common knowledge among the actuarial profession that the person who is a mere mathematician can not be trusted to interpret satisfactorily actuarial facts; the actuary must be an excellent mathematician, but he must also be able to use medical and other knowledge. We can now generalize. Statistical method has very important mathematical aspects, but is not a branch of mathematics.

It has frequently been true that the only course of instruction in statistics given in some of the higher educational institutions has been classified in the catalogues as a branch of economics, and has consisted mostly of economic statistics interwoven with some statistical method. This is not satisfactory in the case of the student interested

in statistical method and in some of its applications outside the field of economics and sociology, but not interested in these two sciences. For this course there should be substituted two distinct courses, statistical method and economic statistics. Anybody who teaches statistical method ought both to know well statistical method and to have a good general knowledge of the entire field of applied statistics; it is assumed in this article that general statistics is so defined that it means both statistical method and general applied statistics. It is necessary to know general statistics in order to select the statistical methods which have most practical value, and in order to classify correctly in the libraries the writings on general statistics.

It is proper to regard economic statistics as a branch of economics; other special branches of statistics should be classified in like manner. But how should general statistics be classified?

In the small college or small university, it is proper if statistical method is taught by a man in the mathematics department, the economics department, or some other department. But the great size of the field of applied statistics should be made apparent. General statistics and statistical method should be classified in the catalogues as an independent department of instruction, and not as a branch of mathematics, economics, or sociology.

How should general statistics be classified by the libraries? Although psychology is not to-day usually taught by a man in the philosophy department, psychology according to "Classification and Index" by Dr. Melvil Dewey is a branch of philosophy. In like manner, general statistics ought to be classified by the libraries as a branch of philosophy. Statistical method according to Dewey is classed as a branch of sociology. It is beyond the purpose of this note to suggest an extensive revision of his Classification. Therefore, I will suggest how it can be made consistent with this note with a minimum of revision. One might admit that it is proper to class statistical method as belonging to general logic, but might argue that general statistics contains too much to be classified as belonging to general logic. When a book deals with two closely allied subjects, it can be classed with the more important which can be regarded as including the other; in other words, classifications are frequently not perfect. In like manner, Dewey's classification ought to be revised so as to classify general statistics as belonging to general logic.

John L. Roberts.

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REVIEWS AND NOTES

An Examination of Logical Positivism. By Julius Rudolph Weinberg. London: Kegan Paul, Trench, Trubner & Co., Ltd. New York: Harcourt, Brace & Company, 1936. Pp. vii, 311.

A comparison of Dr. Weinberg's book with Ayer's *Language, Truth and Logic* inevitably suggests itself. The latter successfully attempts a statement of the position of the Viennese Circle, a friendly exposition of a finished philosophic position, without reference to the line of its development, the problems out of which it had its genesis, the questions it leaves untouched, the difficulties it originates; a sort of diminutive *summa*, or doctrinaire text. Dr. Weinberg's book, *per contra*, is a *critique*. He who runs will not be able to read it. It does not simplify by exclusion or suppression; Dr. Weinberg uncovers all difficulties, some of which he solves *for*, and some *against*, the logical positivists. Ayer's book is an introduction; Weinberg's book, as the title indicates, is an *examination* (an examination both *of* and *for* logical positivism).

Following the Introduction, in which Dr. Weinberg attempts a clarification or definition (more generally the latter) of some of the terms frequently found in the works of logical positivists, and a brief historical survey of the precursors of the Viennese Circle (19th century empiricism, Frege and Russell), the author treats of the following: Part I—logical foundations, in which Wittgenstein's theory of meaning is explained and criticised. The criticism is undertaken within the frame of reference of logical positivism itself; for, the author says, "if there are fatal difficulties with Logical Positivism, they will be discovered by a logical analysis of this system without reference to any philosophical theories" (p. 68). Part II—this treats of the theory of scientific method (probability, induction, and the nature of natural law). Part III—this treats of the elimination of metaphysics, linguistic solipsism, and the positivistic theory of knowledge. Part IV—this treats of radical physicalism (logical syntax of languages, problems of scientific method and philosophy in terms of logical syntax, the physical language, and a critique of physicalism). The Conclusion embodies

an estimate of the Viennese Circle and considers the possibility of an alternative theory of language.

The author accepts the elimination of metaphysics as a *fait accompli*; Hume and Kant, he believes, accomplished this before the logical positivists. A deductive metaphysics is theoretically impossible; beside observation and experiment, non-logical statements are not verifiable. Logical discourse and experiment are the only realms in which we can look for sense; non-logical statements can be tested only by empirical experiment; hence verifiable non-logical statements with a metaphysical reference are impossible. This conclusion of the logical positivists Dr. Weinberg accepts, but he nonetheless questions, and, I think, with justice, the specific doctrines of Wittgenstein on which it is made dependent. Weinberg questions the correctness of Wittgenstein's delineation of the nature of propositional meaning, verification, and logical proposition, but this much he accepts: "no explanation of propositional meaning and truth will allow a significant place in discourse for the assertions of metaphysics."

Concerning the other large contribution of the logical positivists, namely, their theory of scientific concepts and methods, Weinberg expresses considerable doubt concerning its value as a logical schematism or as a basis for a workable scientific method. The demonstration of the ultimate simplicity of the constituents of facts is, he says, a *petitio principii*, and it is useless as a basis for scientific method because it results in solipsism, "and because the limitations it places on language are too narrow to permit a satisfactory expression of general laws and of those scientific concepts which cannot be exhaustively referred to experiential data. And the theory is self-stultifying because it includes in the scope of the nonsensical the philosophical discussions on which its own conclusions depend." The author's argument that Wittgenstein's position leads to linguistic solipsism, and that this result is incompatible with "the minimum requirement of objectivity in science," seems unanswerable, and, if unanswerable, is fatal to the pretensions of the group.

From this brief notice it is not to be assumed that Weinberg is an unsympathetic critic, like McGill or Ritchie. His study is eminently fair, and he ever tries to separate the wheat from the chaff. There is no wholesale praise or unmitigated condemnation. The book does credit to the meticulous scholarship of Dr. Weinberg, his breadth of knowledge, his analytic acumen, and (a rare quality these days in the field of philosophic criticism) his philosophic temper. Its quiet dignity, slow

pace and heavy burden recall Hooker's *Ecclesiastical Polity*. It is not subterranean or grovelling, nor breathtakingly flighty, but sanely pedestrian: the steps are measured and the destination known.

MILTON R. KONVITZ.

The Einstein Theory of Relativity. By H. G. L. R. Lieber, 253 Clinton Ave., Brooklyn, New York. Pp. 73, with illustrations.

This little volume has drawn warm praise from Einstein, Birkhoff, Keyser, Carmichael, Kasner, Bell, Robertson and Pegram. The adjectives used include,—"entertaining," "unique," "amusing and instructive," "very artistic." The text is written by Lillian R. Lieber and looks like free verse, interspersed with easy algebra; the drawings are by Hugh Gray Lieber, very modernistic and pedagogically effective. By all means get a copy.

W. M. M.

BOOKS RECEIVED

Abnormal Personality and Time. By Nathan Israeli. The Science Press Printing Co. Pp. 123. $2.50.

Bishop Butler and the Age of Reason. By Ernest C. Mossner. The Macmillan Co. Pp. 271. $2.50.

The Language of Economics and Ethics. By Robert S. Hale. Bruce Humphries. Pp. 154. $2.00.

The Logical Structure of Science. By A. Cornelius Benjamin. Kegan Paul, Trench, Trubner & Co. 1936. Pp. 344. 10/6.

CORRECTIONS TO HAILPERIN'S "FOUNDATIONS OF PROBABILITY IN MATHEMATICAL LOGIC." SUPPLEMENT, JANUARY, 1937

p. 129, line 6, read "p′, q′" for "p, q"
p. 133, 6th from bottom, read "2.2" for "2.3"
p. 142, 7th from bottom, read "3.93" for "3.92"
p. 142, line 1, summation extends only to end of first parenthesis
p. 144, 17th from bottom, delete "(k)"
p. 147, 15th from bottom, read "logic" for "probability"
p. 150, 14th from bottom, insert "$p \supset q$" between v and first bracket